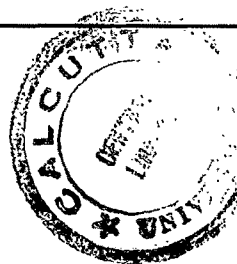


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The American Sociologist

Volume 13 Number 1 February 1978



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EDITOR'S PAGE

This special issue on "alternative theoretical perspectives" in our discipline has been some time in the making. The idea was first broached at the meeting of the Editorial Board held in New York in 1976, in the context of discussions about the meaning of our charge to address "professional concerns of sociologists as a social collectivity." It was argued that there are things going on in the profession which some of our readers do not seem to know very much about, and that they are not likely to learn about from reading the research journals of the discipline, mainly because articles in those latter journals already assume familiarity with the basic theoretical stance from which an author writes.

The possibility of "primer" type treatments of some alternative theoretical perspectives was discussed for some time in correspondence among Board members. Concerns were expressed about what might constitute such perspectives, about dangers of appearing to endorse particular positions, and so on. Those concerns were ultimately resolved, Scott McNall agreed to edit this special issue, and an announcement soliciting papers was published in the journal last spring. Individual announcements were sent out at the same time.

In the four or five months following our May issue, we received more than fifty submissions for the special issue, raising our submission rate far above its usual level. While a small number of papers were sent to us by authors who had been encouraged by one or another member of the Board, most "came out of the blue." It is apparent that there are quite a few among us who believe that they have things theoretical to share. We used more than one hundred special readers; many of them read as many as five or six papers and one of them (to whom special, if anonymous, thanks are owed) read nearly half of all those submitted. Sometimes readers disagreed sharply on the merits of individual papers (as McNall notes in his prefatory note), but there was also agreement that a number of the papers were interesting, novel, and provocative. We have not been able to include all the papers which our readers recommended we publish.

The papers in the pages below do *not* constitute a report on the current status of sociological theory. Our emphasis on "alternative" perspectives excluded, by definition, the more familiar perspectives treated in standard textbooks and introductory courses on theory. Thus, there are no reviews of functional perspectives, or of their now familiar conflict alternatives, and no articles on exchange, attri-

bution, or dissonance "theories." We would not argue, of course, that Marx will be a new name to our readers, or that the notion of "ethnomethodology" is completely unfamiliar. The authors of our articles on these topics, however, along with our referees, appear to agree that contemporary Marxist theory, ethnomethodology and the other perspectives represented here are neither widely understood nor widely viewed as providing viable alternatives to those listed above.

Given this definition of theoretical *alternatives*, I do not fully share the sense of frustration manifested in McNall's comments. I view our authors as saying, "Don't you think you should consider the following?" rather than, "I have here the ungarbled word—and if you'll just pay attention you'll find a direct route to answers of previously unanswerable questions." But that does not mean that I believe that all the perspectives suggested for consideration are equally persuasive. In some instances I find myself in quite sharp disagreement with a perspective, at least as presented. In all cases, however, the papers have been endorsed by highly regarded people in the field. I believe that each should have a hearing. The ultimate test will be that of explanatory adequacy. Whatever, we hope you have fun reading this issue.

We are all indebted to Professor McNall for his efforts in generating this special issue and bringing it to completion. We thank our special readers. Finally, we are grateful to the University of Kansas for making it possible for Scott to visit Bloomington for an editorial meeting on the special issue.

Other features related to theoretical topics will be forthcoming in the remainder of this volume. We hope to have the long-delayed feature on "sociology and complementary disciplines" in an early issue; there may be a feature on what McNall has called "metatheories"; we expect to have responses to the articles published in this issue. (Someone is reading *TAS*; two colleagues who participated in "exchanges" we have published have reported that they have had more requests for reprints of their comments than for anything else they have published!)

We do not intend, however, to neglect other dimensions of shared professional concern. We have articles or features "simmering" on such topics as ethical issues confronting sociologists and growing problems associated with professional employment. We hope to have more features in the "debate" or "exchange" format; we'd much appreciate reader reaction to movement in that direction.

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A processing fee of \$10 is required for each paper submitted; such fees to be waived for student members of ASA. This reflects a policy of the ASA Council and Committee on Publications affecting all ASA journals. It is a reluctant response to the rapidly accelerating costs of manuscript processing. A check or money order, made payable to the American Sociological Association, should accompany each submission. The fee must be paid in order to initiate the processing of the manuscript.

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ON CONTEMPORARY SOCIAL THEORY*

SCOTT G. McNALL

University of Kansas

The American Sociologist 1978, Vol. 13 (February):2-6

The desire to have a special issue of *The American Sociologist* on "alternative theoretical perspectives" was a very personal one for me. I have long been plagued by the questions of what social theory is supposed to do, whether or not it makes any difference, and if so, to whom. I would like to believe that social theory really does make a difference (at least good theory)—that it is the very thing we use on a daily basis to make sense of both ourselves and the world around us. Yet there has seemed to be so much of it, and some that was completely inaccessible to me. The special issue could examine the question of what now constitutes social theory, and see if what is being done makes sense, that is, if it can be *used* to make our reality clearer to us. What, for instance, was being said by critical theorists, ethnomethodologists, Marxists, systems theorists, and so forth, that made a difference to the whole profession? A considerable amount of labor has been devoted to understanding ourselves and modern society. But do we really understand? Why are there so many different theories, images, views, and statements about the way of the social world?

The response to the announcement of our special issue was, at least to me, puzzling. A number of papers arrived dealing with a very wide range of issues—some of which I would not have put under the heading of social theory. Our reviewers, frequently leaders in a particular area, often gave us diametrically opposed opinions on papers. One would report, "The best thing I've seen in this area; publish as is," while another would be offended that such a piece was even sent out for review. There was, of course, eventual consensus on a number of pieces, but a wide range of response was

characteristic. Readers sometimes agreed that a piece was well written and characteristic of work being done in an area, but did not agree that it ought to be part of the literature. And curiously, even papers which were ostensibly about the same thing had few references in common. These facts led me to a number of troubling conclusions. It seems that members of our profession do not draw on a common body of information. They do not have common standards for judging the worthiness of an article. In fact, they disagree about what "theory" is. I think there are several reasons for this.

Looking over the submissions, and simply skimming old journals and books on theory, it seems possible to make a rough distinction between three types of contemporary social theory. First, there is formal theory: either deductive systems, in which explanation (or prediction) consists of deriving lower order propositions from higher order ones; or systems of articulated empirically based "propositions." There are quite a few of these systems in sociology, ranging from Parsonian "action" theory through "exchange" theory and others of the "middle range," to axiomatic "theories" of deviance, suicide, criminology, and so forth. General systems theorists, exact theorists, network analysts, and their brethren continue to refine their work in the hope of discovering laws about human behavior. These theories do not dominate in the discipline.

A second way of conceptualizing theory is to see it as Mannheim (1936) did. A theory is a belief system, or an ideology. Sometimes these world views are rooted in particular classes, and will obviously change from one historical period to another, as when Marx alluded to the fact that the dominant ideas of an age are the ideas of the ruling class. These theories are about how we *ought* to view the

* These brief remarks have benefited from discussion with my colleagues Joe Harry and Gary Howe.

world. They could even be called metatheory. (Followers of Kuhn [1962] could argue that the shared paradigms of a scientific community represent metatheories.) Under this heading we find humanistic sociology, Black sociology, sociology which stresses a feminist perspective, and so on. These may not be explanatory systems, but they can be powerful models for interpreting the order of the world.

Finally, some social theories are a way of making practical sense out of the world, particularly our individual lives. Theory is a form of imagination, in the sense that Mills' (1959) used the concept. For Mills, there are three basic questions with which sociologists should concern themselves. The first has to do with the structure of industrial society: what are its essential components, how are they related, and how does our society differ from others. Second, where does our society stand in human history, and where and how is it changing. Finally, what types of men and women prevail in society and what types will come to prevail. The answers to such questions help the individual to understand his or her personal biography. Social theory from this perspective simply means "making sense of things." This fits Gouldner's (1970) criterion for the test of a good theory—the gut-level feeling that the explanation makes sense. Goffman's (1959) work, or Erikson's (1977) recent book on how people behaved after a disastrous flood fall into this category.

Ideally, a fully satisfactory social theory will encompass all three of these approaches, simultaneously providing us with a formal system, an explanation of a particular socio-political order, and of our location within that order. None of the perspectives in this issue seem to me even to approach that ideal. In my view, this failure is due partly to epistemological problems, and partly to historical factors. First I want to consider some of the roots of this situation.

Social theorists of the Enlightenment believed, among other things, that humankind would progress because of a combination of human (which also meant humane) reason and empirical methods. This distinction eventually led to a split

between the objective and subjective realms. Social theorists often pursued only one realm of human existence at considerable cost to their theories, and to their understanding of man. In Durkheim's work, for instance, there is a general dissociation of subject and object. However, modern sociology has carried this method to its logical extreme, in the areas of both theory and method. Survey research represents such an extreme of the mind-body division, as does the elevation of methodological techniques to the status of theories (as sometimes occurs in discussion of path analysis).

Other social theorists seem to have abandoned the social world as objective reality. Aristotle and Plato argued that one became an individual through society. In contrast, some contemporary thinkers argue that one becomes an individual in spite of society. This celebration of the self is represented in the United States by subjective social theories, such as ethnomethodology, which are based on the philosophies of hermeneutics and phenomenology (though some may argue that Garfinkel [1967] is attempting to forge the links between the objective and subjective realms). It should be noted that this preoccupation with the self is primarily an American idiosyncrasy; among continental social theorists, hermeneutics deals with events beyond the individual.

The difficulties generated by the epistemological problems are exacerbated by the historically specific bases of social theories. Mannheim holds that ideologies vary from one historical moment to another; our own observation shows that theories have a distinct political appeal. Often social theorists are caught up in their own history, and their "theories" become statements of personal belief. It should surprise nobody that "new" sociological theories began to flourish during the 1960s. It is not clear that any of them (or any of the older theories which were resurrected, such as those of the rediscovered continental theorists, particularly those members of the Frankfurt School who had settled in New York) explained the times, or our anxieties. Some of the trends which emerged in that period were reflected in the submissions to this

issue, e.g., a preoccupation with problems of political legitimacy, a general debunking of values and tradition, and a concomitant celebration of the self. Hermeneutics, phenomenology, and ethnomethodology certainly reflected this trend.

So far, I have separated historical and epistemological problems in examining contemporary social theory. Can this really be done? Given that modern man finds himself in a schizophrenic dilemma, then the epistemological problem becomes a translation of that dilemma and cannot be separated from it. If modern man is truly divided against himself, then our theories must reflect this, and struggle with it. One of the purposes of social theory must be to provide modern man with the means to critique the social order, and thereby to achieve a modicum of freedom.

There have been, and continue to be, heroic efforts to bridge the objective/subjective gap in social theory. Marx's system clearly involved an understanding of the individual situated in a historical setting, and his concept of *praxis* specifically bridges the gap between the objective and subjective realms. I also believe that this was Weber's intention when he spoke of *verstehen*. Like the phenomenologist, Weber regarded it as important to study the ways in which individuals, constrained by others, manage to act; like Marx, he believed in the importance of the historical context.

Appelbaum (1978:76), in discussing the power of Marxist dialectics, notes that

What distinguishes Marxism from positivist social science is its ability to move simultaneously on two levels: it formulates the laws of the "natural history" of capitalist economic organization, and at the same time demonstrates the ideological (i.e., historically situated) character of those laws so that they might be repealed by self-conscious workers organized collectively in their own interests.

Sociology has drifted considerably from what Marx, Weber and other major social theorists saw as the mission of sociology.

Are there any really new social theories? The issues are still the same. Consider the submissions for this issue:

many were attempts to clarify older theories, e.g., Marxism, functionalism, dialectics, and so forth; or they were comments about what ought to be emphasized, e.g., biology or the emotions; or they were attempts to apply old theories in new areas, e.g., Marxism and the city. There is a growing tendency in sociology to talk about how we ought to perceive reality, and not about the nature of reality itself. A consequence of this is an inability to *do* theory—to create a system to explain the social order, and to test that system. Perhaps we have enough social theory. Whatever their faults, Parsons, Durkheim, Marx, Weber, Simmel, Toennies, Tocqueville, and Veblen did provide us with total images of reality.

Of course, it is not easy to create a theory and to examine it under the harsh light of social reality. Marx, Weber, Durkheim, and others devoted lifetimes to clarifying, applying, changing, and reformulating their visions of social order. Their systems have transhistorical validity because they all tried to consider the relations and contradictions between such things as the political, economic, cultural, educational, and familial structures, as well as man's biological nature, in a historical setting. Today's theories are about specific, and increasingly limited, portions of reality which cannot be integrated with one another. There is little, if any, indication that theorists are aware of the historical moment and its effects on perception, or of what the epistemological issues are. It is appropriate, and necessary, to deal with limited portions of the social order, but that cannot excuse failure to deal with the larger questions. It may be necessary to answer small questions one at a time in order to approach greater issues, but we cannot get big answers to small questions.

The other extreme poses its problems too. Global cosmologies are as bad as atomized empiricism. We need to seek the reality that lies between. Nisbet (1976) has suggested that there are a number of themes common to the social sciences (community, masses, power, and so forth), and that these themes extend across the ages. They comprise a central part of major social theory, and must be dealt with anew in each historical period.

New theories are not really built on the theories of the past in the way that people normally think of advancing knowledge.

... Of all the Idols of the Mind of Profession regnant today the worst is that which Bacon might have placed among his idols of the Theatre: the belief, first, that there is something properly called theory in sociology, and second, that the aim of all sociological research should be that of adding to or advancing theory. (Nisbet, 1976:20)

The grand theories of Comte, Spencer and Ward are in the dead past. What remain are the *insights* gleaned from the use of those theories. For Nisbet, theory is the illumination, the sense of discovery, that accompanies any genuinely fresh study of a piece of the world we live in. That is why Simmel's *Secret Society*, Riesman's *The Lonely Crowd*, Goffman's *Asylums*, Anderson's *Passages from Antiquity to Feudalism*, and Wallerstein's *The Modern World System*, fall under the heading of theory. They provide us with fresh insight. According to Nisbet, this is the best that sociology can aspire to. The function of social theory is to provide us with new portraits: surreal, post-Modern, it does not matter. Once we have seen them, the ways in which we perceive reality are forever changed. "What artist of the period gave us role-types in his novel or painting more evocative than what we draw from Marx about the bourgeois and the worker, from Weber about the bureaucrat, or from Michels on the party politician?" (Nisbet, 1976:7). I agree with Nisbet about what theory should do, and does. His idea is an ideology; it also makes sense of things. Where do the papers in this issue stand, then? What do we learn from them about social theory in the United States?

I began this enterprise with a belief, namely, that social theory should make sense to individuals by allowing them to retrospectively explain their biographies; and an assumption, namely, that sociologists have something to say about the dilemmas of humankind in the modern world. Are this belief and this assumption grounded in the realities of contemporary sociological theory? I think not.

Behavioral sociology, general systems theory, and the like illuminate small por-

tions of reality. They could address large issues, but they do not. Critical theory, which sensitizes us to the concepts, images, and language we use in constructing theory, fails to provide a grounding, and often results in a deep pessimism about the possibility of change. Ethnomethodology offers useful insights into the means by which rules are constructed, but is often ahistorical and fails to deal with problems of power. The varieties of Marxist sociology call our attention to the inherent contradictions within capitalist states, and to some of the ways in which the system guarantees its own survival. But its images of human beings are not as valuable as its dialectics, which could provide a means of understanding how total systems operate. Finally, a new environmental paradigm tells us to pay closer attention to the interrelatedness of certain phenomena.

All of these theories are limited. Obviously, if their authors had had more space they could have shown how the theories could be applied in more ways. Yet the very attempts to formulate theories in a short space are illuminating. The space constraint subjects the theory to one of Weber's ultimate tests: meaningful adequacy. Does the theory, when applied by someone else, make sense out of reality?

There are a number of new perspectives which are not represented in this issue, but should be. Humanistic sociology, feminist sociology, and perhaps Black sociology are about ways in which reality can or should be perceived. There are important variations on the theories which do appear here, and other areas which are as yet unexplored.

But these, too, will provide only a piece of the puzzle. The question is, rather, whether most sociologists even understand that there is a puzzle to be put together and made sensible. Do most sociologists really know what is going on, theoretically, outside of their own specialty? All of these alternatives offer insights, along with a clearer understanding and appreciation for what constitutes contemporary social theory.

I noted earlier that there have been attempts to bridge the epistemological gaps within the discipline. My own preference

is for a combination of critical theory and Marxist structuralism: critical theory, because it sensitizes us to the process of theory construction, how we perceive reality, and the fact that theories are political; Marxist structuralism, because it sensitizes us to the historical realm and to the contradictions inherent in modern life, and it also provides us with a viable image of what can be. Other theoretical perspectives can be most illuminating by beginning with the questions that plague modern man and dealing with them in such a way that they make personal sense.

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ETHNOMETHODOLOGY*

DON H. ZIMMERMAN

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This brief essay attempts to make certain strands of ethnomethodological thought intelligible to a general audience in the hope of opening this growing tradition to those who are interested and who may find it of some use in their own work. The discussion focuses in turn upon the relationship of phenomenology to ethnomethodology, the issue of reductionism, the concept of "natural language," the relationship of context and particular in ethnomethodological work, and on the possibility of interchange between ethnomethodology and other sociological approaches.

Ethnomethodology, like sociology itself, encompasses a number of more or less distinct and sometimes incompatible lines of inquiry. The term is often applied to the work of persons who, if consulted on the matter, might choose other designations, e.g., phenomenological sociology or conversation analysis. Unfortunately, few commentators seem to appreciate the increasing diversity among

ethnomethodologists, with respect to choice of both problem and method (Mehan and Wood, 1976).

Garfinkel (1974:18) asserts that the term has become a shibboleth, and elsewhere (Hinkle, *et al.*, 1977:9-17) intimates that attempts to clarify the "program" and define the boundaries of ethnomethodology have deflected attention from serious research. While it is difficult to contest the importance of doing research rather than talking about it,¹ there is nevertheless the

* Revised version of a paper presented at the 1976 Pacific Sociological Association Annual Meeting, San Diego. I would like to thank Douglas Maynard, Candace West, Thomas P. Wilson and four anonymous readers for their many helpful comments and suggestions.

¹ Whether or not the energies of ethnomethodologists have been largely devoted to programmatic statements is open to question. While

problem of providing access to the area for those interested in it. Moreover, since ethnomethodology has been the target of a number of critiques (cf. Armstrong, 1977) that may be more familiar than the primary literature in the area, passing attention to certain of these criticisms is necessary in order to furnish a more reliable guide to understanding and appraising ethnomethodological inquiry.

The aim of this paper, therefore, is not to define ethnomethodology, or to provide an authoritative reply to its critics. Instead, this brief essay will attempt to make certain strands of ethnomethodological thought intelligible to a general audience, thereby, perhaps, to open this growing tradition to those who are interested and who may find it useful in their own work. Discussion will focus, in turn, on phenomenological sociology, the issue of reductionism, the concept of "natural language," the relationship between context and particular in ethnomethodological work, and, in conclusion, on the relationship between ethnomethodology and other concerns in sociology.

Phenomenological Sociology: Intellectual Heritage and Intellectual Content

Critics frequently assume that ethnomethodology is a phenomenological

sociology. This often leads to critiques of phenomenology rather than critiques of ethnomethodology, and thus to obfuscation of more pertinent issues. Critical surveys such as Attewell (1974), Goldthorpe (1973), and Mayrl (1973), all proceed on the assumption that ethnomethodology is a phenomenological sociology. Mayrl (1973:15) flatly asserts that "ethnomethodology represents an attempt to develop a systematic program of research based upon phenomenological premises." He writes:

[Ethnomethodology's] theoretical thrust is entirely consistent with Schutz's postulate of subjective interpretation and its use of meaning in no way contradicts the concept of that phenomenon as a quality of individual subjectivity . . . their methodological stance leads quite logically to idealism and ultimately solipsism . . ."² (p. 27)

The work of phenomenologists, and in particular that of Schutz (1962, 1964, 1966, 1967, 1970a, 1970b), has indeed figured prominently in the development of ethnomethodology. However, Giddens (1976a), in discussing critics of Durkheim, makes a relevant and very important point:

We may, and ordinarily must, distinguish between the intellectual antecedents of a man's thought, the tradition he draws upon in framing his views, and the intellectual *content* of his work, what he makes of the ideas he takes from the tradition. (pp. 710-711)

In the present context, the question is: What has happened to phenomenological ideas in the transformation from Schutz to Garfinkel, Sacks, and other ethnomethodologists?

First of all, there are many enterprises that could be called phenomenological in some sense of the word, such as Weber's emphasis on the importance of the meaning of behavior to the actor. Others claim to use a phenomenological method, and of these, some—as shown by Heap and Roth (1973) (but see also Wieder in Hinkle, *et al.*, 1977:105)—are misguided or naive.

² I do not intend to imply that, if ethnomethodology were phenomenological through and through, these critiques would be correct. It is not clear, for example, that a phenomenological stance leads inevitably to solipsism (cf. Bittner, 1973; Wieder, 1977).

Garfinkel's (1967) classic statement contains definite programmatic elements, *Studies in Ethnomethodology* is a collection of empirical studies over the course of which Garfinkel developed his conception of ethnomethodology. Sacks (1963) published only one frankly programmatic statement. There are, of course, publications which can be viewed in this way, e.g., Zimmerman and Pollner (1970) or even Cicourel (1964). There is at the present time one book which might be viewed as a text (Mehan and Wood, 1975). While other empirical and theoretical works may contain programmatic remarks, it seems evident that the bulk of published work by persons who could be identified as ethnomethodologists of one stripe or another (as opposed to commentaries on ethnomethodology provided largely by critics) consist of actual empirical research (e.g., Bittner, 1967a, 1967b; Cicourel, 1968, 1973; Cicourel, *et al.*, 1974; Emerson and Pollner, 1976, in press; Jefferson, 1972, 1973; Jefferson and Schenkein, 1977; Garfinkel and Sacks, 1970; McHugh, 1968; Pollner, 1974, 1975, in press; Schegloff, 1968; Schegloff and Sacks, 1973; Sacks, 1972a, 1972b, 1972c, 1973, 1974, 1975; Sacks, Schegloff and Jefferson, 1974; Sudnow, 1965, 1969, 1972; Wieder, 1970, 1974; Zimmerman, 1969, 1970a, 1970b; Zimmerman and Wieder, in press).

Others are more sophisticated; for example, the work of Bittner (1973) and Wieder (1974, 1977). Work in this last vein might properly be called "phenomenological sociology," for it proceeds from a thorough grasp of technical phenomenology and seeks to develop an approach to studying society that is informed by phenomenological concepts and methods. Wieder (Hinkle, *et al.*, 1977:4-5) argues, for example, that:

The import of the phenomenological method . . . is to give direct access to the world of immediate experience in terms of intending (the acts of consciousness) and the objects intended . . . through these acts. As I see it, it would be the task of a phenomenological sociology to describe and explicate such intended objects as the . . . experience ordinarily referred to by way of the concepts social role, norm, institution, cultural object, the other person, motives, language, and the like. Furthermore, such a discipline would describe that which is distinctive about the intentional acts which present these objects to consciousness.

While ethnomethodology might be called "phenomenological" in the weak sense that the work of Schutz and other phenomenologists was a significant antecedent (cf. Heap and Roth, 1973), and while some ethnomethodologists may find the writings of phenomenologists relevant to their work, treating ethnomethodology as a whole as a phenomenological sociology risks a number of errors, among them the assumption that ethnomethodology necessarily employs phenomenological methods, or that its varieties are united, as Coser (1975: 698) put it, by a "celebration . . . of the transcendental ego." (It should be noted that the latter characterization does not even pertain to phenomenological sociology.)

One particularly grotesque consequence of viewing ethnomethodology in this way is the treatment accorded the work of Sacks and his associates. Their work, while heavily influenced by Garfinkel, bears the mark of linguistic and anthropological thinkers (for example, Chomsky, 1959, 1965; Goodenough, 1965) and linguistic philosophers (such as the later Wittgenstein, 1953; Austin, 1961, 1965) rather than specifically phe-

nomenological sources. For example, Attewell (1974) tells us that Sacks' enterprise involves recourse to the realm of the "transcendental ego"; according to Mayrl (1973), Sacks was engaged in furnishing an "eidetic description of eidetic description," whatever that is supposed to mean. Even a casual reading of Sacks' work would dispel such claims.

In the development of ethnomethodology, ideas originally inspired by deep familiarity with the work of Schutz and others have undergone major changes, e.g., Garfinkel's (1967:262-283) transformation of Schutz' (1964) analysis of rationality into an empirical sociological problem. His appropriation of Schutz' (1964:3-96; 207-259) description of the "natural attitude" for the study of common sense understanding of social action (1967:35-75) might also be noted. Strictly speaking, the term "phenomenological" is inappropriate as a blanket characterization of the working tools, methods and problems of ethnomethodology, if for no other reason than that it blurs the distinction between intellectual heritage and intellectual content.

Reductionism

A critic might argue that even allowing some degree of "discontinuity" with the phenomenological tradition, and granting the distinctions between various phenomenological sociologies or among ethnomethodologists, ethnomethodology nevertheless remains, in general, a form of subjectivism in social science, and can be read as denying the possibility of a science of society. This is so, the argument might go, because ethnomethodology denies that social facts are exterior and constraining, or alternately, that society as an objective entity conditions social life at the everyday level; hence, individual members of society, operating in a context of few if any constraints, simply decide how the world should be as a matter of mere preference. Moreover, since ethnomethodology treats "meanings" as indexical rather than transsituationally stable, as implied in the notion of "cognitive consensus" (cf. Wilson, 1970; Parsons, 1951), it denies the intersubjective character of

culture, and is reduced to cataloguing particular meanings on an ad hoc basis, a phenomenologically inspired but sociologically aimless empiricism.

This characterization of ethnomethodology is an example of the old controversy between "holism" and "individualism" in history and social science, used here in the sense of the opposition between "society" and "individual." Even if it is conceded that ethnomethodology is not concerned with the constitution of the social world in the individual consciousness, it could still be charged that "society," i.e., the institutional order, is being reduced to the various understandings, perceptions, beliefs, lay-theories, attitudes and the like employed or invoked on concrete occasions by individual members of society. That is, society is "reduced" to a "mind-originated" and "mind-dependent" phenomenon, as, for example, in the case of dreams (Jarvie, 1972:153-154). From this perspective, ethnomethodology is either a form of radical subjectivism or, what is little better, a species of psychologism. In fact it is neither, as the following considerations suggest.

How are such objects as "individuals" referred to in the context of ethnomethodological writings? In some cases they are called "actors," but the more theoretically relevant term is "member-of-society" or simply "member." The term "member" has shifted in meaning over time. The earlier more Parsonian usage took the form of "collectivity member" (cf. Garfinkel, 1967:76; Parsons, 1951:41-98). More recently the notion refers not to persons as such but to "mastery of the natural language" (Garfinkel and Sacks, 1970:342). "Natural language" should not be construed in a narrow sense, e.g., as the syntax and semantics of, say, a specific language; rather, the notion refers to a system of practices

that permit speakers and auditors to hear, and in other ways to witness, the objective production and objective display of common sense knowledge, and of practical circumstances; practical actions and practical sociological reasoning . . . (Garfinkel and Sacks, 1970:342)

For present purposes, what is important about these two ways of regarding individuals—the first in terms of obligations and constraints imposed by collectivity membership, the second in terms of constraints imposed on speaker-hearers by the properties and practices of natural language—is that reference to an individual necessarily implies a supra-individual system, a form or forms of social organization in terms of which the notion "individual" becomes intelligible. Such a system is clearly *intersubjective*. It involves shared skills or procedures by which particular events are organized into instances of an external social order. In this sense, "members" (or alternatively, speaker-hearers) are *agents* of the system in question. That is, the activities of individuals are of interest only insofar as their activities exhibit the workings of the system. In this restricted sense Giddens (1976b:5) is correct when he proposes that ethnomethodology places

the notion of agency once more in the forefront of sociological theory . . . the thesis that human society as produced by human individuals is a creative and skilled production, that the sustaining of even the most trivial kind of encounter between individuals . . . is a creative phenomenon of the same order as the speaking of a language is a creative phenomenon.

The notion of "creativity" should not, however, be confused with free will or like notions which imply that persons "freely" create—out of whole cloth—the society that encompasses them. Instead, this is creativity in Chomsky's (1959) sense, when he speaks of the creative character of linguistic competence, i.e., the ability of speaker-hearers to produce and recognize an indefinitely large number of novel sentences through the use of a finite set of elements and rules for their combinations. Creativity, however, is both possible and occurs within the context of the constraints such rules provide. To adopt such a view of "agency" and of "creativity" does not deny the consciousness of the individual, or transform the person into a mere automaton, but instead *specifies* a particular kind of interest in the activities, capacities, and perceptions that characterize individuals.

Mandelbaum (1959) has suggested a criterion of irreducibility, i.e., a means of testing the claim of methodological individualism that all societal level concepts can be translated into concepts referring to the properties of individuals. Mandelbaum's criterion asks whether or not

... those concepts which are used to refer to the forms of organization of a society [can] be reduced without remainder to concepts which only refer to the thoughts and actions of specific individuals. (p. 479)

It is Mandelbaum's view that many forms of behavior in society cannot be reduced to the properties of individuals "without remainder," i.e., without presupposing or making reference to an institutional order in terms of which the meaning of the behavior is defined. For example, the concept of "natural language" cannot be reduced to individual terms "without remainder," any more than the more restricted term "English language" could be so reduced. "Natural language" is, in some sense, a societal level concept, or at least a supra-individual notion. An often neglected implication of Mandelbaum's argument is that in many respects the properties of "individuals" themselves are not readily defined without reference to societal or supra-individual notions. For example, how can reference be made to an individual's "thoughts" without presupposing some language in which thoughts are thought—or at least expressed. In any event, it does not follow that by rejecting an absolute transcendence of "society" over "individual" ethnomethodology necessarily embraces a radical subjectivism—any more than having phenomenological antecedents necessarily entails a phenomenological commitment.

Natural Language

It was suggested earlier that "natural language" is not defined by reference to linguistic categories such as syntax and semantics, although those aspects of language structure are potentially relevant to any research into the properties of "natural language." However, linguistic

units such as "sentence," when studied in isolation from their pragmatic contexts, suppress what is of prime interest here: the use of natural language expressions in interactive situations. Indeed, an actual utterance cannot properly be viewed only as a more or less flawed production by a speaker employing his or her grasp of the rules of sentence construction. Instead it must be seen as an *interactional object*, subject not only to syntactic and semantic constraints in the narrow sense, but also to the properties of speaker-hearer interaction (cf. Goodwin, 1975) and, among other things, to the constraint of turn-taking and to repair-systems for conversation interaction as formulated by Sacks, Schegloff and Jefferson (1974). Giddens (1976b), a non-ethnomethodologist, suggests that ethnomethodology has called attention to

the significance of language as a medium of practical activity . . . [This notion] deviates rather basically from the [idea of] the language as a series of signs or symbols. . . . The significance of [this] new view of language is that language is a practical medium of the accomplishment of practical social tasks, that is, that language is a mode of doing things. (p. 6)

Ethnomethodology is not, of course, unique in the view that "language is a mode of doing things" (cf. Wittgenstein, 1953; Austin, 1961, 1965). Sociolinguistics, and in particular, the approach called the "ethnography of speaking" (see, for example, Bauman and Sherzer, 1975; Gumperz and Hymes, 1964; Hymes, 1962, 1974; Philips, 1976), share with ethnomethodology a concern with language-in-use as a social and interactional object to be observed *in situ* and described in its own right rather than merely as a resource for studying social life. Natural language is, in this view, basically social. Ethnomethodology differs from other approaches to language in society in the conception of natural language as a system which is (1) prior to and independent of any particular speaker, that is, external, and (2) less preferential than obligatory, that is, constraining. As a system, natural language exhibits the properties of the Durkheimian social fact, although such properties are themselves

the accomplishment of members using the system on actual occasions of interaction. Moreover, in contrast to the emphasis on social variation characteristic of the ethnography of speaking (e.g., Bauman and Sherzer, 1975; Hymes, 1974; Philips, 1976), natural language is considered to be a widespread general and abstract system which operates on any local context to order the particulars of talk and action into patterns which reflect both the "immediate" social reality and a "transcendent" social reality beyond that local context (Zimmerman and Pollner, 1970).

In particular, the study of "natural language" involves the systematics of producing utterances, expressions, gestures, and so forth, which (a) achieve a particular meaning or delineated range of alternative meanings in some local environment; (b) contribute to, establish, negotiate or expose a "definition or definitions of the situation"; or (c) express and warrant assertions or statements concerning one's or the other's "state of mind," "motive," "feeling," what's right or what's wrong with the world, and so on. These are seen as situated accomplishments of the use of "natural language" (cf. Garfinkel and Sacks, 1970) and are of interest only insofar as they lead to a fuller understanding of how the system which produced them works. Of course, a focus on the properties of "natural language" does not entail limiting inquiry to what members "know explicitly" about their own activities and the conditions of their interaction, any more than the linguist's informant can produce a linguistically acceptable grammar of his or her language.

Contexts and Particulars

Another important issue must be addressed: ethnomethodology's insistence on the "situated," "contextual," or "embedded" character of social activity—including within the domain of the latter term the activity of describing social activity. Ethnomethodology proposes that the properties of social life which seem objective, factual, and transsituational, are actually managed accomplishments or achievements of local processes. Some critics (cf. Coser, 1975) apparently con-

sider this sufficient to claim that ethnomethodology is primarily concerned with a theoretical description of particular meanings, and that it eschews—or even denies the possibility of—generalizing beyond such narrow and particular events. Doubtless, persons calling themselves ethnomethodologists could be found who take such a view. Nevertheless, a generalizing intent lies behind the ethnomethodological concern for the particular-in-context. Ethnomethodology studies on-going social activity in order to discover the properties of the social organization of natural language which provide for the accomplishment of definite meanings, convergent definitions, warranted accounts, all in the lively context of their occurrence. Obviously, to extract an event such as a member's statement from the locally organized context in which it occurs, without knowledge of the principles of that local organization, runs the risk of fundamentally distorting the information carefully garnered through coding procedures or other research tools.

An example modeled after Cicourel (1973:11–41) may serve to illustrate these remarks in a limited way. Sociologists employ the concepts of status and role to refer to more or less institutionalized patterns of conduct. As Cicourel (1973:11–24) observes, a distinction is often made between these concepts, status being considered the more institutionalized of the two. Simply stated, however, status and role entail rules of conduct which define appropriate conduct in specified situations; they are independent of particular incumbents; and, to the extent that they are deemed effective guides for conduct, they are coercive. Roles must be brought to bear in actual situations, i.e., they must enter in some way into the activity of persons subject to them. The general pattern of action indicated by the role must be employed by an actor in some setting: it must be fitted to the setting, called into play at the appropriate time, interpreted in light of past and present events and the reactions of other participants, and so forth. Role, of course, is not only a prescription for conduct; it is also a scheme of interpretation. Our own and others' activities are recognized, eval-

uated, praised and criticized in terms of roles. Moreover, social actors are subject to a multiplicity of role demands, and hence, must be thought of as entering into a sequence of roles, presumably organized in terms of particular local settings.

It would surely be odd if a society were designed so that its institutions were partly constructed of role-relationships, but lacked any systematic mechanism for articulating societal roles within the features of various interactional settings (cf. Cicourel, 1973:33-39; Wieder, 1974; Sacks, *et al.*, 1974; West and Zimmerman, 1977; Zimmerman and West, 1975, 1977). It would be stranger still if this articulation were itself not socially organized. Strangest of all would be a state of affairs in which the instantiation of a role in an actual situation had no bearing on the understanding of roles in general, or the sense of "objectivity" and transcendence of the role. Ethnomethodology posits a reflexive or, perhaps, a dialectical relationship. A widespread, abstract, and general form of social organization—the constituent practices of "natural language" (Garfinkel and Sacks, 1970)—is available as a resource for the accomplishment of society and biography in local contexts; individuals ("members"), as agents of this massive, socially organized system, do employ those resources; and the activities summarized here are public, observable, controllable events which require no empirically uncontrolled reference to "mind" nor any special modes of access to the private or subjective.

Concluding Remarks

In this paper I have outlined a particular way of understanding certain aspects of ethnomethodology, which other ethnomethodologists may not share. The requirement of brevity, moreover, brings with it an inevitable sketchiness that can also mean distortion. Yet, my aim is not to preserve the purity of the enterprise, and with it, the choosing up of sides. Just as ethnomethodology has used phenomenological writings without necessarily committing itself to the intellectual problems and disciplinary constraints of

phenomenology, so too the work of ethnomethodology could be used by other approaches without limiting them to its particulars. As Garfinkel has suggested (Hinkle, *et al.*, 1977:10-13) there is an important difference between the pursuit of problems within a tradition of thought (where the problems will have their own, distinct, local complexion), and the use of a tradition to reflect upon, speculate about, and imaginatively play with research issues that have an altogether different frame and countenance. This is not a license for the wholesale distortion of any body of work. The pertinent distinction is between scholarly accuracy in dealing with ideas in their intellectual context and insistence on doctrinal purity. The former may be applauded, the latter can be done without.

Which brings me to my last point. Ethnomethodology is not a comprehensive theory of society as the latter term is understood. It is an approach to the study of the fundamental bases of social order. That is, ethnomethodology is concerned with those structures of social interaction which would be invariant to the revolutionary transformation of a society's institutions. Thus, it has not directly concerned itself with such issues as power, the distribution of resources in society, or the historical shape of institutions. Nevertheless, the possible interchange between ethnomethodology and more "macrosociological" concerns is being explored more and more (cf. Chua, 1977; Collins, 1975; Mehan and Wood, 1975: 205-224; Molotch and Lester, 1974; Smith, 1974a, 1974b, 1975; West and Zimmerman, 1977; Zimmerman and West, 1975, 1977). Whether or not cross-fertilization is possible between, for example, recent Marxist approaches to sociology (cf. Appelbaum, *in press*, and in this issue) and ethnomethodology is an open question, and an intriguing possibility.

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CRITICAL THEORY AND THE CRITIQUE OF CONSERVATIVE METHOD*

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In this essay I will examine several aspects of the writings of the Frankfurt School. First, I will consider the detrimental effects of positivism in the social sciences, and the alternative offered by critical theory. I will examine the central concept of the societal totality as seen by members of the School. The work of Juergen Habermas has been instrumental in the contemporary development of critical theory. I will briefly describe attempts to make critical theory more directly applicable to and involved in social change. Finally, I will consider some of the problems generated by these attempts, and some future directions for critical theory.

There has been a general reawakening of interest in historical interpretations of society in contemporary Anglo-American social thought. In particular, Marxism has reemerged as an important social scientific endeavor—partly as a result of problems of social development in the second half of the twentieth century, including changes in the world political situation. At the same time, Marxist thought has developed in very diverse ways; e.g., the various works of Lukács (1971), Gramsci (1971), Korsch (1971) and more recently, the anti-Hegelian-Marxist perspectives of

Althusser (1970), Colletti (1973), and Therborn (1976), have attracted much international attention.¹ One of the most important and original strains of neo-Marxism has come from the work of the members of the Frankfurt School, whose intentions were: (1) to distinguish their form of Marxist thought from the mechanistic economic determinism and

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¹ For a useful account of these various trends within Western Marxism see Howard and Klare (1972). See also Anderson (1976). Much of the debate regarding these authors' contributions to Marxist thought have taken place in the pages of the British journal, *New Left Review*, and also in *Telos* (published at Washington University), and *New German Critique* (published at the University of Wisconsin, Milwaukee).

evolutionism of "official Marxism" of the internationals; (2) to emphasize the significance of cultural factors and of theoretical criticism itself in the development of society; and (3) to mark their affinity with a philosophical, rather than a purely "scientific" and positivist formulation of Marxism. (For an excellent account of the history of the Frankfurt School and an extensive bibliography of the publications of its major figures, see Jay, 1973.) In this paper I will summarize and analyze the core theme in the theoretical program of the School, that is, the establishment of "critical theory," as found in the work of the three original members, Max Horkheimer, Theodor Adorno, and Herbert Marcuse. I will also consider the most prominent "reformulation" of critical theory found in the recent work of Juergen Habermas, and discuss some critical responses to the theory.

Critical theory is both a critique of society and a critique of the theory of knowledge by which society is known. In order to understand it, I will approach it first as an alternative to positivism in the social sciences and then as a critique of the political organization and culture of advanced industrial society.² More pragmatically, critical theory will be considered as a way of escaping technocratic domination and control, and reinstituting reason. The Frankfurt School project for reason is a total intellectual orientation which requires no less than a complete redefinition of: (1) the purpose of social inquiry; (2) the scope of legitimate knowledge of social and political phenomena; (3) the role of the theorist; (4) the relation of theory and practice; and, (5) the relationships between facts and values.

Positivism

There is no generally accepted usage of the notion of positivism; scholars of different orientations and in different times have used it to suit their own theoretical requirements and intellectual or ideological predispositions (see

Habermas, 1971; Giddens, 1975; Frisby, 1976). In the practice of social science the term has generally been used to refer to the incorporation of natural science methods into that practice. Three assumptions are implied by this notion of positivism: (1) since the methodological procedures of natural science are used as a model, human values enter into the study of social phenomena and conduct only as objects; (2) the goal of social scientific investigation is to construct laws, or law-like generalizations like those of physics; (3) social science has a technical character, providing knowledge which is solely instrumental. It follows from these assumptions that knowledge is unfinished and relative and, because social scientific knowledge is neutral with respect to values, that the knowledge has no inherent logical implications for policy. The categorical distinction between fact and value prohibits the social theorist from taking a normative position or advocating what "ought to be"; his/her role as a social scientist is confined to formulating and testing propositions about reality, and does not include advocacy or social action.

Critical Theory

What is missing in such a formulation is attention to "the *critical* role of theory—or perhaps more accurately . . . the problem of the role of critical theory" (Giddens, 1975:18). From a positivist perspective, the task of philosophy is to provide clarity for scientific statements; critical theorists see this as reducing philosophy to methodology. According to the Frankfurt School, this view necessarily enhances a technological system of domination (this point will be developed below). More is involved in this dispute than the issue of proper methodology in the social sciences—the very aim of social science is being questioned. Horkheimer clearly outlined the distinction between critical and traditional theory forty years ago (1937, reprinted in 1972; see also Lukács, 1971, for a similar notion) when he said that for "theory in the traditional sense . . . the social genesis of problems, the real situation in which science is put to

² For a discussion of critical theory as contrasted to phenomenology and hermeneutics, see Dallmayr and McCarthy (1977), Giddens (1976, 1977).

use, and the purposes which it is made to serve are all regarded by science as external to itself" (1972:244). He contrasted this with critical theory which "has for its object men as producers of their historical way of life in its totality" (1972:244), and in which "Objects, the kind of perception, the questions asked, and the meaning all bear witness to human activity and the degree of man's power" (1972:244).

Extending his critique of traditional science to bourgeois society, Horkheimer argued that treating the *object* of inquiry as distinct from the subject of inquiry results in a system of technical rationality which effectively blocks rational political practice. Because it restricts the description and categories of science to that which is (the facts), positivism views history as a "natural" process governed by natural laws rather than as a human process capable of being influenced by human activity. (Indeed, Marx's entire critique of bourgeois political economy stands as a methodological reflection upon critical social science, and, as such, has direct relevance for current methodological controversies. See Marx, 1973:81-111.) Knowledge of social reality becomes "neutral" information which can be integrated into the existing social structure. By separating facts and values and consigning the latter (as *sense-less*) to the arbitrary and subjective realm of politics, positive science becomes incapable of challenging (as science) the existing system, and thereby becomes conservative. Science becomes an activity far removed from the sphere of practical or moral action except in terms of assessing the adequacy of means toward *given* ends. Such a stance, in the words of a contemporary critical theorist,

supplies the social engineers of the industrial system with the legitimation of measures in accordance with the dominant value system, which is withdrawn from any effective public discussion: this means—in accordance with the stabilization of the existing social power structure. (Wellmer, 1971:21)

Adorno (1976:109), elaborating on the differences between traditional and critical theories, criticizes the former for its approach to problem solution. By criticizing proposed solutions on the basis of

existing evidence, positivists appear to accept and reify existing social reality. The net consequence of assuming a positivist stance toward society as the object of knowledge is to leave that object unquestioned and to accept its substantive rationality. In a debate with Popper (1969), Adorno (1976) has argued once again that the instrumentalist (positivist) approach to reason artificially separates social scientific knowledge from the object it studies. Marcuse (1964), in a pessimistic analysis of advanced industrial society, states that critical theory is distinguished by refusal to accept the given (i.e., positivist) universe of facts as the final context of validation. Acceptance of knowledge which is derived uncritically from empirical facts allows continuing reproduction of existing relationships in society.

Reification of basic social institutions and of conventional activities is a *logical* and socially crucial consequence of technological practice (positivist social science). A political decision-making apparatus based on such practice (and such social science) will necessarily treat some aspects of the basic structural relationships of the society being studied as *given* (beyond critical or evaluative analysis) (Fay, 1975). In advanced industrial societies, for example, the decision-making process will be constrained by the primary goal of the efficient functioning of the processes of production (for an elaboration of this point see Offe, 1976).

The Societal Totality

While, as Horkheimer stated, the "denunciation of what is currently called reason is the greatest service reason can render" (1947:187), critical theory has refused to limit its activity to methodological criticism and clarification. By asserting that traditional scientific methods do not constitute an adequate foundation for valid knowledge, critical theory obviously questions the adequacy and relevance of a traditional experimental method applied to the "testing" of empirical propositions about social phenomena (this is not to argue that the School abstained from empirical research; see below). Critical

theory replaces the rejected notion of individual social facts with the concept of societal totalities, observing that empirical experiments which abstract a problematic phenomenon from that totality can never successfully illuminate critical understanding (Frisby, 1972:113; Jay, 1977). I will briefly trace how this totality has been envisioned by members of the Frankfurt School.

From its beginnings, the Frankfurt School has focused on the epistemological and philosophical dimension in Marxism. During the early years (1923–1929), however, under the leadership of the historian Carl Gruenberg, a critique of political economy was central to the concerns and activities of the School. The School's role was seen as integral to problems of the workers' movement—bourgeois science was not regarded as a serious challenge or as of particular interest or concern. The collective goal was the illumination of the objective possibilities for destroying class society.

When Horkheimer became Director in 1930, the orientation of the Frankfurt School changed, in part because of changes in society, in part because of differences in the scholarly orientations of Gruenberg and Horkheimer. Whereas Gruenberg had been a socialist labor historian relatively unconcerned with the philosophical and theoretical bases of Marxism, Horkheimer was a philosopher, concerned with the development of a social philosophy supplemented by empirical investigation. While Horkheimer was a political radical, he had never been a member of a working-class party.

This shift away from the traditional Marxist concern with political economy and proletarian revolution became clear after the School was forced to flee Germany to Geneva in 1933, and to Columbia University in 1934. The principal factor in this shift of emphasis was the disappearance of the revolutionary historical "subject" of traditional Marxism—a militant industrial proletariat. The reorientation was in part an attempt to formulate a perspective which would explain new historical conditions, namely, the consolidation of capitalism and the integration of the proletariat into that consolidated

capitalism, phenomena signalled by the rise of the Third Reich, Fascism, and the concomitant decline of the militant labor movement. The change further reflected a philosophical rejection of the economic determinism of orthodox Marxism. The energies of the Frankfurt School were turned increasingly to the study of "mediations" between consciousness and being, psychoanalysis, the structure of authority, the emergence of mass culture, and aesthetic issues.

While Marxist thought continued to be central to the scholarly orientation of the Frankfurt School, during the American years the Frankfurt thinkers were heavily influenced by Freud's work on the mechanisms by which individuals are integrated into society. This attention to Freud was paralleled by a shift toward greater emphasis on understanding modern culture. In particular, the Frankfurt School scholars were interested in applying Freud's insights to analysis of the psychological mechanisms by which authority, repression and domination are internalized and transmitted. Members of the group devised innovative empirical research techniques, and were among the first to apply social-psychological categories to the analysis of working class attitudes, mass society and culture, patterns of authority and family relations, the sources and nature of prejudice, the role of women in modern society, and the genesis and structure of Nazism (see, for example, Neumann, 1944; Fromm, 1968; Adorno *et al.*, 1950; Komarovsky, 1940; Bettelheim and Janowitz, 1950; Lowenthal, 1957). These studies indicate the Frankfurt School's refusal to relegate the cultural superstructure, or societal totality, to a secondary role in the analysis of modern society.

The entire Frankfurt tradition (from Horkheimer to Habermas) has constituted a sustained attack on positivism because it implies a subordination and capitulation to the reality of existing social forms, namely, capitalism. The common interest of scholars in this tradition has been to overcome the "eclipse of moral and political standards" brought about by the instrumental rationality of modern scientific thought. In order to achieve this, the

Frankfurt thinkers have attempted to: (1) recover the notion of substantive rationality through a critique of ideology (a critique which permits the illumination of the realities of social life and social structure which are hidden by the distortions of positivistic thought); and, simultaneously (2) provide a "material critique" which will inform revolutionary human activity oriented to the creation of emancipating social conditions, thereby enabling humanity to seek and achieve better lives. In short, critical social theory's purpose is the merger of reason and action, reflection and commitment. To this extent critical theory necessarily has a "practical intent." However, this merger is not free from both theoretical and practical dilemmas, nor do critical theorists themselves agree on how to accomplish the tasks before them. How is critical theory to become a concrete factor in the practical, radical transformation of society? How is it to become more than an empty abstraction which periodically issues condemnations of existing reality? Habermas has given much thought to these issues.

Juergen Habermas

Habermas is the most recent and most controversial figure to emerge from the Frankfurt School's "great refusal" to "speak the language of domination." Drawing upon the diverse and opposing traditions of Marxist-Hegelianism, empiricism, phenomenology, hermeneutics, systems theory, and the modern philosophy of language, Habermas has reformulated the foundations of a critical theory of society in order to develop a more comprehensive alternative to a naturalistic understanding of social inquiry.

Habermas has given new form to the demarcation between traditional and critical theory (see esp. Habermas, 1971). In his discussion of the relationships between cognitive endeavors, and human experience and purpose, Habermas identifies three different types of knowledge and "knowledge-constitutive interests," i.e., interests which orient humans (unknowingly) in their relations and reflections upon reality: (1) the *emancipatory*

interest of the cognitive activities of critical theory; (2) the *technical* cognitive interests of the empirical-analytic mode of thought; and (3) the *practical* interests of the hermeneutic mode of thought. The empirical sciences approach reality from the perspective of technical control and manipulation of objectified processes (as technically exploitable knowledge). Hermeneutic inquiry is oriented toward the basic "practical" interest of human communication between social individuals.

Habermas has withdrawn somewhat from the orthodoxies of earlier Frankfurt thinkers. His critique of empirical analytic reasoning is less sweeping. At the same time, he is more cautious and skeptical than both his predecessors and some of his contemporaries about the emancipatory potential of contemporary Marxism (see Wellmer, 1971, for another contemporary representative of the Frankfurt School). Marcuse, in particular, states that empirical reason is always destructive—not only when it is employed for barbaric ends. To Marcuse, it is not a matter of the object to which empirical reason is applied; empirical reason necessarily and inherently examines its object (society) from an instrumental perspective, which accordingly reduces its object to objectified processes to be controlled, manipulated, and dominated. Social oppression is thus seen as the triumph of instrumental reason (positivism); therefore, social liberation must entail a transformation of scientific reason itself (Marcuse, 1964, 1969b).

Habermas believes that Marcuse has confused the logic by which physical nature can be understood with the logic by which people understand each other. He condemns the original Frankfurt School for falling prey to "speculation" and the "heritage of mysticism" (Habermas, 1971:32–33). Habermas (1970a) singles out Marcuse as a "romantic utopian" calling for a new science and technology to create a "new sensibility" between nature and man. Although Habermas is firm in his critique of domination by technological rationality, he wants to preserve the logic of science as such (as instrumental calculation). He believes that reason should not be *limited* to empirical analytic

cognition. Marcuse and Horkheimer, in contrast, want to eliminate positivism as the carrier of a repressive social order.

Critical Theory and Social Change

The relationship between reason and emancipation has direct implications not only for the critical theorist's understanding of the sources of oppressive social relations, but also for the practical task of social change. In 1843 (reprinted in 1975), Marx wrote that a revolutionary theory of society was not yet a "material force" capable of "gripping the masses" and overthrowing the capitalist order. Marxism is still grappling with the problem of how to transform a theory into a concrete historical force, embodied in a self-conscious revolutionary subject (the industrial proletariat). Critical theory must determine the "concrete roads" (Marcuse, 1968) leading its agents of revolutionary praxis to the realization of a just society.

Horkheimer (1972:218, 225), writing during the late nineteen-thirties, apparently accepted the world-historical role ascribed to the proletariat by Marx, although he clearly felt the need to maintain some distance between the proletariat and their scholarly ideologists. In the midst of the turmoil and destruction of the period, Horkheimer's continuing adherence to the classical Marxist model of social change seems somewhat surprising to some critical theorists today. Indeed, Adorno's later work contains a sharply contrasting view of the agent of change (cf. Horkheimer and Adorno, 1972). For Adorno, the proletariat and the possibility of a rational society exist only as "concepts" (Adorno, 1967:19, 1973:148-151). The task of critical theory is reduced to preserving such notions (or concepts) as justice and freedom, in order to illuminate the extent to which existing reality falls short of these ideals. This model of "immanent criticism" lacks immediate practical consequences for anyone except the critical theorist observing the contradictions of bourgeois society. Marcuse (1969b) also abandons the guiding principle of the unity of theory and practice. He looks beyond the proletariat for a transitional agent (cf.

Marcuse, 1972). Recognizing that "... critical theory is left without the rationale for transcending this society" (1964:xiv), Marcuse turns to a new agent for change. He argues for a "new sensibility" and a "new science," to become the imaginative vehicles for the forces of liberation. Marcuse thus fails, as did Adorno, to avoid the trap of empty abstraction.

Habermas is also aware of the problem of connecting critical theory with the actual practice of the human agents of social change. The emancipatory interest of critical theory, according to Habermas, provides all members of society (not only the proletariat) with knowledge of those repressive social conditions which constrain rational autonomous action. The model *critically* adopted by Habermas for the practical realization of critical theory is Freudian psychoanalysis (see Habermas, 1970c, 1971, 1976). Habermas finds within psychoanalysis an emancipatory project of therapy. The analyst "gets behind" experiential explanations offered by the patient in order to explain (causally) distorted or repressed meaning patterns that have become inaccessible to the consciousness. The goal is to further self-reflection and self-knowledge in the patient and, ultimately, to explain and remove unnecessary forms of social domination (see McCarthy, 1973). Transferred to the social and political realm, psychoanalysis links causal explanation and subjective interpretation: through undistorted dialogue between participants in an ideal speech situation (mediated by the psychoanalytic method), Habermas claims to have located the ground on which cognition and action can be joined, thus providing critical theory with its practical outlet.

The Problem of Application

Abstractness also plagues implementation of Habermas's project. The process of therapy is accomplished through symbolic communication, in which both parties agree to work toward the emancipation of the patient. But what if the material conditions for this Socratic dialogue are not given? What if there are recalcitrant par-

ticipants in the discursive process—clearly the rule in human history? As Bernstein (1976:224) has noted, “what seems to be lacking here is any illumination on the problem of human agency and motivation . . . What are the concrete dynamics of this process: Who are or will become its agents?”³ Emancipation must obviously be more than either a symbolic exercise or the mere recording of distorted communication. We still lack a positive formulation of what it is that enables human beings to overcome repressive social conditions.

Critical theory continues to be perplexed, moreover, by ambiguity regarding its empirical status—what one observer calls its “twilight zone” (Dallmayr, 1976). Is the critique of society based on an understanding of “facts”? Does it take place on the level of some transcendental understanding? Indeed, the very project of bridging the gap between what is and what ought to be necessarily imposes a certain abstractness—an abstractness made even more acute by present social conditions which have increasingly repressed any viable radical political opposition (critical theory’s human agency). The concept of the societal totality lacks clear delineation. A fully adequate critical theory, then, has yet to be formulated. However, these fundamental inadequacies are due more to the programmatic character and incomplete development of critical theory, than to doctrinaire assertions claiming a “direct pipeline to reality through science, revelation or metaphysics” (Piccone, 1977:195).

What, then, can critical theory offer sociology today? Despite the above criticisms, sociologists cannot afford to ignore critical theory’s questions about the nature and purpose of social inquiry. The insights into the potentially barbaric consequences of scientific rationality are

neither capricious nor ideologically motivated. It is vital that sociologists engage in critical self-reflection about what they are actually doing. The critical theorist would argue that there is no such thing as value neutrality or objectivity in sociology, nor should there be. We must examine the relationship between the practice of social scientific research and how our knowledge about social life affects the evolution of that life. Although there are some unresolved (and perhaps unresolvable) difficulties in the application of a critical theory of society, the issues are still significant. Ultimately, critical theory is about the fundamental purpose of knowledge and social theory; and that is its real value for sociology.

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³ For some useful discussions regarding this notion of critical theory and its practice, see Jay (1972a, 1972b, 1973), Krahll (1974), Lubasz (1975), Piccone (1976, 1977). For criticisms from members of the Althusserian school who identify critical theorists as “historical humanists” who reduce the Marxist “science of political economy” to a “metaphysical humanism,” see Althusser (1970), Therborn (1970). Also see Slater (1977), Anderson (1976).

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ERRATUM

An error appeared in the "Rejoinder" by Lee Ellis, *TAS* 12 (November, 1976). The first sentence of the final paragraph on page 198 should read as follows:

So far, the main concern of sociobiology has been to articulate a broad explanation for *why* social behavior is found in so many different species and forms.

BEHAVIORAL SOCIOLOGY: EMERGENT FORMS AND ISSUES

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Behavioral sociology has developed unique features which have caused controversy and confusion in the discipline. In this paper, we briefly describe the history of behavioral sociology, and show how its major forms have been applied to behavioral analysis in complex organizations, laboratory experimental analysis of social exchange and social process, and behavioral macrosociology and theoretical extensions. We also briefly examine criticisms of behavioral sociology on grounds of tautology, reduction, the omission of internal states as relevant data, and behavior control.

We wish to provide an introduction to behavioral sociology by briefly describing its origins, major forms, and the features that have made it attractive to some, but unattractive to others. Although we cannot describe all that falls under the broad heading of behavioral sociology in this brief paper, our intent is to provide a representative view of a major perspective that, according to Ritzer (1975:vi), remains unfamiliar to most sociologists.

Ritzer (1975:145) defines behavioral sociology as the "theoretical effort to apply the principles of psychological behaviorism to sociological questions." Thus, a behavioral sociologist is one who uses the concepts and principles of operant conditioning for the purpose of examining, explaining, and sometimes altering human social behavior. Behavior analysis may be applied to the socially relevant behavior of individuals, the interaction of individuals in groups, or the behavior of groups, organizations, or even societies as units. Behavioral sociologists are interested in how the social behavior of individuals or collectives is influenced by social and nonsocial environments.

ORIGINS AND DEVELOPMENT

The basic concepts and principles used by behavioral sociologists are those of operant conditioning, empirically derived by B. F. Skinner and his students (e.g., Skinner, 1953). Operants are behaviors that "operate" on the environment, producing some consequence which may alter sub-

sequent behavior. In short, operants are behaviors that are maintained or altered by their consequences. The functional relationships between operants and their consequences (relationships which are usually expressed as contingency statements) provide the foundation for operant analyses and behavior modification. Behaviorists generally explain behavior by specifying the conditions that reliably produce it. This means identifying the consequences (i.e., reinforcers and punishers) of behavior, their sources, and the contingencies or conditions on which they are based. Identification, in turn, frequently requires the experimental manipulation of consequences and contingencies to effectively control the behavior under examination. Thus, distinctions between explanation, prediction, and control become somewhat blurred. Although the principles and procedures of operant conditioning cannot be presented here (see Reynolds, 1975 for a brief introduction), it is important to note that behavioral sociologists have gotten much mileage from only a few basic principles.

Ritzer (1975:142) considers Skinner the major exemplar of social behaviorism (not to be confused with Mead's social behaviorism). But George Homans (1961), directly influenced by Skinner, stimulated the adoption of these principles by developing and elaborating five general behavioral propositions which form the foundation for his social exchange approach. Perhaps the major significance of this early attempt was that it provided the

groundwork for the transition from controlling the nonsocial behavior of pigeons to analyzing the social exchange behavior of humans. This transition was based partly on the simple observation that people in interaction establish contingencies for rewarding and punishing one another. However, Homans clearly indicated that the explanatory power of these principles was not confined to face-to-face interaction, but extended to macrosociological phenomena as well. In fact, according to Homans (1964:818), the only *general explanatory* propositions are "not about the equilibrium of societies but about the behavior of men."

Although Homans's writings may not have led *directly* to behavioral sociology as it appears today, he was adopted as sociological exemplar by a group of sociologists primarily at Washington University. This group (which also included Robert Burgess, Don Bushell, David Schmitt, and James Wiggins) had independently become interested in applied behavior analysis under the influence of Keith Miller and later of Robert Hamblin. They were instrumental in developing behavioral sociology as it appears today. One substantial contribution was Burgess and Bushell's (1969) book (which included contributions from Homans, Kunkel, and Skinner, as well as from the Washington University group), which coined the label, defined the forms and scope of the perspective, and still serves as a major textbook for courses in behavioral sociology (see Hamblin and Kunkel, 1977, for more recent readings on behavioral theory and research in sociology).

MAJOR FORMS

Research and writings described in this section are divided into three categories: (1) applied behavior analysis in complex organizations; (2) experimental analyses of social exchange and social process; and (3) behavioral macrosociology and theoretical extensions. These categories correspond to work done primarily by psychologists, social psychologists, and sociologists, respectively.

Applied Behavior Analysis in Complex Organizations

Although complex organizations constitute a major analytical domain within sociology, few sociologists have become involved in, or even paid attention to, applied behavior analysis in such settings. Applied behavior analysis began by examining and altering the idiosyncratic behaviors of individual mental patients; its application has gradually shifted to normals, to complex social behavior, and toward total systems (e.g., see Goodall, 1972, for a brief overview).

Behavior analysts have worked with "captive subjects" in mental hospitals, as well as in educational and correctional settings. In all such settings the token economy is the most popular systemic program. For example, Hamblin and his colleagues (Hamblin, *et al.*, 1971) used a token economy system to alter the behavior of inner-city school children who had been labeled culturally deprived, disturbed, and unteachable. Students were awarded tokens for meeting previously defined behavioral objectives. Tokens earned could be exchanged for a variety of goods or opportunities. Bizarre and disruptive behaviors were treated by terminating reinforcement for the behaviors (extinction procedures) and simultaneously reinforcing other, more accepted social behaviors. Although occasionally a "time out" procedure (brief removal of the opportunity to earn tokens) was used, the use of aversive stimuli or punishment was avoided. Among the behaviors strengthened were reading, writing, correct language use, good work habits, peer tutoring, constructive interaction with teachers and peers, and even better performance on IQ tests.

Since Nord (1969) outlined the transition from educational to management applications, similar reinforcement procedures (though not token economies) have been used to strengthen attendance, punctuality, and quality and quantity of performance by both workers and supervisors. For example, Emery Air Freight was able to substantially improve sales; responses to customer queries, and

appropriate container usage by instituting a worker administered performance feedback system, and by reinforcing performance improvement with praise and recognition (Business Week, 1971).

A recent text (Nietzel, *et al.*, 1977) suggests that behavior analysis can also be applied at the community level to a wide range of problems including drug abuse, alcoholism, community mental health, aging, unemployment, and the environment. Applications aimed at reducing environmental problems (littering, recycling, energy conservation, and pollution, for example) show particular promise for immediate adoption on an even larger scale (e.g., see the review by Tusso and Geller, 1976).

It is easy for the sociologist to dismiss applied behavior analysis on the basis that, although behavioral, it is not sociology. After all, the principles and procedures used were developed and are applied primarily by psychologists. Furthermore, the fact that the analyses occur in organizational and community settings does not make them inherently sociological. On the other hand, both the settings and the problems addressed have also been analytically addressed by sociologists. Thus, there are several reasons why the application of behavior analysis should at least be regarded as sociologically relevant. First, these applications go beyond descriptive and correlational analyses to deal with variables that directly affect behavior; therefore, they may provide a more complete understanding of organizations and communities. Second, because these settings constitute major analytical domains within sociology, sociologists should have something to contribute. Third, the more sociologists do become involved, the more the applications will involve social structural variables, and thus become more sociological. Finally, as funding becomes more contingent on demonstrable utility, applied sociology may become more attractive.

Experimental Analyses of Social Exchange and Social Process

*Recent behavioral social exchange formulations combine operant principles and

procedures with the early social exchange frameworks of Thibaut and Kelley (1959) and Homans (1961). In one of the first attempts in this area, Emerson (1972) used balanced and unbalanced power-dependence relations between actors to address such traditional sociological concerns as values, cohesion, norm formation, role and status, stratification, and division of labor, as applied to both small and large groups.

The more recent social exchange formulation by Molm and Wiggins (1977) is even more explicitly operant, using a strictly operant model rather than a rational choice model. They have investigated the conditions under which social exchange is established, maintained, disrupted, and re-established. Other laboratory-experimental analyses of social exchange which address the same concerns include those by Burgess and Nielsen (1974), Marwell and Schmitt (1975), and Michaels and Wiggins (1976).

The major independent variables in most of these studies involve structural power-dependence relations between actors which produce variations in profit, inequity, and social exchange. Procedural paradigms involve a series of trials during which subjects choose to obtain rewards independently, through exchange, or cooperatively.

The work of Marwell and Schmitt (1975) best exemplifies the cumulative nature of this research; they have done more than 30 interrelated experiments on factors affecting cooperation. Two persons in separate rooms could earn money by operating plungers on consoles. Both could earn money cooperatively only if *both* pushed their switches to "Work with other person" and operated their plungers for mutual benefit. Otherwise, each could earn money (usually somewhat less) by pushing the switch to "Work alone" and operating the plunger solely for individual benefit. Factors found to interfere with cooperation included payoff inequity, risk, and the opportunity to "rip-off" one another. Factors found to facilitate cooperation included unconditional cooperation by other, the opportunity for verbal communication (visual presence had no

effect), and, of course, higher payoffs for cooperation.

Other laboratory-experimental analyses of social process have examined macrosociological concerns using larger groups. For example, Wiggins (1966) examined some of the functions of the stratification issue raised by Davis and Moore by controlling how status differentiated groups allocated group rewards (differentially or equally) as a function of the external consequences of the allocations. Leik, Emerson, and Burgess (1968) examined social stratification by controlling the internal stratification of groups as a function of structurally determined exchange outcomes of various coalitions. Finally, Burgess' (1968) experiments on problem solving communication networks resolved long-standing inconsistencies regarding the relative efficiency of various networks. In these experiments, the Circle (a decentralized network) and the Wheel (a centralized network) were found to be equally effective when reinforced for speed and accuracy of solutions, and when the groups operated until steady state performance was reached. During early trials without reinforcement, however, the Wheel was superior.

But are such studies sociological? Not if sociology is restricted to the analysis of existing social systems in their natural settings. However, if sociology involves the analysis of general social processes such as stratification and functional differentiation, such studies are definitely sociological. It is important to note, however, that such studies should be regarded as demonstrations of how the behavior of social systems can be controlled by exogenous consequences and contingencies; not as explanations for how any particular social system got to be the way it is.

Behavioral Macrosociology and Theoretical Extensions

According to Friedrichs (1974:4-5), if behavioral sociology is to make the headway he predicts for it, it will demand a subtler mediator than Skinner or Homans. In terms of empirical research, he nominates for this role James S. Coleman,

whose work involves both a policy research orientation and mathematical models appropriate to the macroscopic settings and problems faced by sociologists. However, although Coleman's (e.g., 1973) work does involve elements found in social exchange formulations (e.g., rational choice and profit maximization assumptions), he explicitly disavowed any connection with Skinnerian psychology during his address at a plenary session of the ASA convention (Montreal, 1974). Robert Hamblin may be a more willing nominee. His previous work may be considered from the behavioral perspective, and his recent mathematical-empirical research falls into the same tradition. Unlike Coleman, Hamblin uses inductively derived (curve fitting) mathematical equations and an explicit reinforcement interpretation in his analysis of cultural innovation and diffusion (Hamblin, *et al.*, 1973). His first step in explaining "use diffusion," for example, is to find the equations (usually exponential or logistic) which best describe the diffusion of activities (e.g., number of automobile registrations, academic degrees conferred, marriages and divorces, and various leisure activities) over time. Although these equations yield the expected high level of explained variance (median r^2 is .98), many will no doubt consider the equations descriptive rather than explanatory. However, these equations are then integrated into an axiomatic mathematical theory with a reinforcement interpretation. For example, the power function equation of Axiom 2 states that the level of behavioral steady state is a positive monotonic function of the level of reinforcement (Hamblin, *et al.*, 1973:198).

In terms of theoretical explications, Friedrichs (1974:6) nominates John Finley Scott (1971) as the exemplar (perhaps because the title of his book, *Internalization of Norms*, gave little hint of its contents—a social learning approach to socialization). Indeed, social learning formulations such as Scott's may be more attractive to sociologists than strictly operant formulations because they provide more familiar elaborations, and they more closely approximate what sociologists regard as relevant theory. In this regard Kunkel's (1970, 1975) social learning for-

mulation addresses such traditional sociological concerns as social structure and anomie, cultural deprivation, social change and economic growth, and social problems.

Theoretical elaboration of behavioral principles has been applied to other areas of traditional sociological concern. Burgess and Akers's (1966a) behavioral reformulation of Sutherland's differential association theory of criminal behavior states, in essence, that criminal behavior is likely to be adopted whenever the opportunities, and magnitudes and frequencies of reinforcement, are greater for criminal than for noncriminal behavior. This reformulation was later expanded (Akers, 1973) as a general social learning theory of deviant behavior that complements other deviance theories such as anomie, conflict, control, and labeling. In another example, Michaels (1974) described the similarities between human ecology and behavioral psychology and considered the desirability and feasibility of linking the two behavioral approaches. Similarities discussed included the recognition of the interaction between behavior and environment, the view that change is externally determined, and quantitative analyses involving aggregated behaviors. Rather striking parallels between the general concepts and principles of the two approaches were also described.

Clearly, behavioral macrosociology has shown substantial variation and elaboration in form and substance, as well as convergence with more established sociological approaches. There are two reasons for this: (1) operant principles lack the cultural substance necessary to explain particular social phenomena; and (2) social systems include characteristics and processes different from those found in individual learning (Kunkel, 1975:186). Thus, emergence, elaboration, and convergence are likely to remain the dominant patterns in behavioral sociology.

ISSUES

*Perhaps no other perspective appears so completely different, even antithetical to traditional sociological training and

practice, as the behavioral perspective. The unique features of this perspective are major attractions for some, but have evoked major criticism from others.

In a recent survey of behavioral sociologists (Green, 1975), respondents were asked to give reasons they were attracted to this perspective. Typical answers were: (1) it is a rigorous scientific approach, methodologically strong, theoretically parsimonious and sound; (2) it makes sense and is a useful alternative for the study of human social behavior; and (3) it is objective, replicable, experimental, and cumulative, and thus offers techniques that can be used to make significant changes in social behavior. The need for a social technology has been expressed by Tarter (1973: 153): "A science without a technology is doomed to the monotony of repetitive observation and idle speculation."

However, other sociologists utterly reject the possibility that behavioral principles have anything to offer sociology. Several features of behaviorism have been issues of concern in psychology as well as sociology. Although space constraints preclude lengthy consideration of these issues, we would be remiss if we failed to consider them, however briefly.

Tautology

There is considerable confusion regarding the logical status of reinforcement principles. There is disagreement not only over whether or not they are tautological, but also over whether the use of tautologies should be admired or condemned. Regarding the first question, reinforcement definitions and principles can be stated in both tautological and non-tautological forms (e.g., Burgess and Akers, 1966b). Regarding the second question, tautologies have been admired for their utility in other disciplines such as physics and economics. If reinforcement principles are tautological, they are relational tautologies, rather than logical tautologies (Chadwick-Jones, 1976:214-218). Finally, a conclusion either way would seem to make little difference at the operational level.

Reduction

Because the term "reduction" has been used in many ways, its meaning for sociologists is not entirely clear. The classical (some say only) case of *complete reduction* was the reduction of thermodynamics to statistical mechanics; yet both still constitute separate disciplines. Because the requirements for complete reduction are very demanding of both sides, sociology and psychology seem unlikely candidates.

On the other hand, Homans, when he attempts to explain or interpret sociological principles by induction, correlating them to more abstract, corresponding psychological principles, is engaging in *logical reduction* (Chadwick-Jones, 1976:367). Although some sociological principles can be "explained" or accounted for in this way, it should be obvious that the consequent reduction will be devoid of the original socio-cultural concepts or referents.

Finally, some critics apparently fear that behavioral sociologists intend to atomize or nominalize emergent phenomena. Yet, as suggested by Kunkel (1975:47), behavioral sociology naturally leads to social structural, rather than individualistic, explanations.

The Omission of Internal States

Behaviorists generally, but not universally, omit internal states as relevant data. The resulting interpretation of behavior appears to move the locus of control from inside the actor to outside stimuli that are directly observable. Thus, autonomous and purposive man is no longer the initiator of his own action, but a responder to external stimuli. Regardless of the utility of such a model of man, it is apparently a discomfoting one for many. Although behaviorists manage without involving internal states, any sociologist bent on explaining behavior by the cognitive processes that precede it can, on this basis alone, rightfully reject behavioral sociology.

The Issue of Behavior Control

Although the principles and procedures of the behavior perspective can be used solely for the analysis of social process, this perspective, more than any other, implies behavior modification and control. London (1969:208), states "The ethical challenge is that of how to preserve or enhance individual liberty under circumstances where its suppression will frequently be justified not only by the common welfare but for the individual's happiness." Kunkel (1975:116) has expressed a somewhat different view. He believes that when there is widespread agreement that a particular problem exists, it makes little sense, moral or otherwise, to leave it as it is.

CONCLUSION

The dominant pattern in the development of behavioral sociology has clearly been one of emergence. Behavioral sociology has shown great variability, elaboration, and even convergence with more established sociological perspectives. It is applicable to both micro- and macrosociological research problems. Thus, we agree with Warland's (1971:576) statement that behavioral sociology may be "best viewed as a critical step toward a future synthesis of theoretical and methodological views in the social sciences which hopefully will produce a more meaningful sociological perspective than our present one."

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TOWARD A SOCIOLOGY OF EMOTIONS: SOME PROBLEMS AND SOME SOLUTIONS

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Psychologists and physiologists have pre-empted the study of emotions because emotions are organismic and psychological phenomena. Nonetheless sociologists have a contribution to make because emotions very frequently result from real, imagined, anticipated, or recollected outcomes of social relationships. In order to study emotions sociologically we require a systematic model of relationship. I propose that empirical results warrant the adoption of a model based on two dimensions: power and the accord of status. I propose that felt excess and/or felt deficit of power and status give rise to a number of distressful emotions: guilt, shame, anxiety, depression, and anger. I propose further that the sociological approach to emotions by way of the power-status relational model helps to clarify a significant controversy in the psychophysiology of emotions, namely whether emotions have specific neurochemical bases, or whether all emotions have a common physiological substrate. A sociological analysis clearly supports the specificity theory. In complementary fashion, the specificity results in psychophysiology also support the power-status model of relationship. The two approaches comprise a basis for a new sociophysiology of emotions.

Durkheim's *Suicide* was a tour de force that showed how sociology could explain phenomena seemingly far distant from it. What could be more private than the inner turmoil, personal despair, or impassioned madness that leads a person to take his or her own life? Surely sociology could have nothing to contribute here. Yet, by his analysis of the *social* forms of suicide, Durkheim illuminated conditions external to the individual that determine the passion for self-extinction.

The study of the emotions is a similar case. Here again the phenomena are private, buried even more deeply in the organism than the reasons for suicide, since the emotions are by general agreement phenomena of the autonomic nervous system, beyond the conscious control of the actor. Thus, emotions appear doubly removed from the social realm. Whereas the actor himself must participate in his own suicide, he has no control at all in the earliest stages of emotional arousal. What, then, is sociology's role in the study of emotions?

My purpose here is to discuss some problems and solutions in developing a sociological theory of emotions. First, I shall sketch briefly some of the current approaches to the study of emotions in psychology. Second, I shall propose a general model of social relations that can

be used to gain insight into the problem of emotions. Next I shall apply the model to the systematic analysis of five distressful emotions: guilt, shame, anxiety, depression, and anger. Finally, I shall discuss the empirical grounds for a theoretical link between sociology and psychophysiology.¹

The Study of Emotions

The systematic study of emotions is mainly in the hands of psychologists. I shall briefly review five traditions of research on emotions in psychology in order to clarify the basis for a sociological contribution. First, many investigations have been devoted to discovering and classifying the sheer number of emotions. This is an old quest and includes the speculations of many philosophers beginning at least as far back as Aristotle (for a useful review of the philosophic tradition see Gardiner, *et al.*, 1937). In the present era of "scientific psychology," the question has been pursued theoretically as an aspect of instinct theory (McDougall, 1933), psychoanalytic theory (Stanley-Jones, 1970), and biological

¹ In Kemper (in press) I treat a large number of questions relating to a sociology of emotions. The brief space of this paper does not permit more than a very few topics to be considered.

adaptation theory (Plutchick, 1962). Empirically, a popular approach involves the study of facial expressions, often by the method of asking research subjects to sort photographs of individuals with different expressions (Osgood, 1966; Ekman, *et al.*, 1972). The results of these studies are mixed; different investigators report different numbers of emotions. The sociologist's response must be to ignore the question of how many emotions there are, and to concentrate on the number of different social conditions—whether as cues, as socialization parameters, interactional and relational outcomes, and so on—which trigger emotions.

A second approach to emotions in psychology uses phylogenetic and innate theories deriving from the work of Darwin (1873), who saw evolutionary continuity in the emotions of animals and human beings. A strong Darwinian influence pervades much of the study of emotions today; in fact, the evolutionary, adaptive perspective has no serious challengers (see Plutchik, 1962; Lazarus and Averill, 1972; and Hamburg, *et al.*, 1975). An important debate in this tradition is whether emotions are innate (e.g., Lorenz, 1966; Wilson, 1975), or learned (e.g., Berkowitz, 1962; Staub, 1971). Regardless of the outcome of the nature vs. nurture argument, the sociological position is secure: either we investigate the social cues that trigger innate emotions, or we investigate the socialization conditions that instill emotions.

A third approach to the study of emotions examines the psychophysiological—brain and autonomic nervous system—causes, concomitants, and sequels of emotion. While this approach would seem to hold the least interest for sociologists since it depends on the technical lore of physiology—a discipline far removed from sociology—, there is a body of work in psychophysiology that remarkably invites, and repays, close attention by sociology. I shall devote the last section of this paper to a consideration of one of the pertinent questions in this area.

Cognitively-oriented psychologists take a fourth approach to emotions. They acknowledge the presence of an external world—often social in nature—which in-

augurates the cognitive processes of “appraisal” or “comparison” that lead to felt emotions (Arnold, 1960; Schachter and Singer, 1962; Lazarus, 1975). But cognitive theorists pay relatively little attention to the dimensional or conceptual nature of the social world, since their concern is with the psychological process. Hence, sociologists can make a vital contribution here. They can provide a conceptual model of the social settings that cue the specific appraisals and comparisons which, according to the cognitive theorists, precede emotions.

Learning theories comprise a fifth distinctive approach to the emotions in psychology. This approach is closest to a situational or sociological analysis. In most learning theories, environmental events are understood to consist of rewards and punishments (or reinforcements). According to Gray (1971:9), “the common element binding the emotions into a class is that they all represent some kind of reaction to a ‘reinforcing event’ or to signals of impending reinforcing events.” Lazarus (1968), for example, proposes that anxiety is the consequence of noxious or threatening stimuli, while depression is a reaction to a real or potential deprivation of positive reinforcements. Similarly, Gewirtz (1969) discusses depressed mood as a response to the absence of environmental reinforcers that ordinarily elicit pleasurable behavior.

The learning theory approach points clearly to a social context for emotions, since it is mainly other actors who provide the positive and negative reinforcements in the course of interaction. The learning theorists, perhaps unknowingly, have taken to heart Durkheim's (1938:10) definition of a “social fact”: it is “external” to the individual and it “constrains” him. Indeed, any individual's reinforcement contingencies are the social facts that elicit the autonomic-motoric-cognitive responses we call emotions. Sociologists, however, are not learning theorists, and need to work with a somewhat more concretely and socially specified set of concepts than merely rewards and punishments. In the following section I shall present what I believe may be a sufficient set of “social facts” to inaugurate a broad

sociological approach to the question of emotions.

There are many theories of emotions; none is specifically sociological. This is not to say that sociologists have entirely neglected emotions. Cooley (1902) identified a particular emotion (the "my feeling") as a fundamental part of the experience of the self. Marx (1964) discussed the "mortification" of alienated labor. Durkheim (1915; 1951) analyzed the religious emotion, and the despair and elation that may both lead to suicide. Weber (1946:215-216) dissected the bureaucratic administrative style: "Its specific nature, which is welcomed by capitalism, develops the more perfectly the more the bureaucracy is 'dehumanized,' the more completely it succeeds in eliminating from official business love, hatred, and all purely personal, irrational, and emotional elements." Indeed one of Weber's four types of social action is the "affectual," or emotional (Weber, 1947:115).

Contemporary sociologists have also touched on emotions: there is Reisman, *et al.*'s (1950) delineation of typical emotions for their several character types—shame for the "tradition-directed," guilt for the "inner-directed," and anxiety for the "other-directed"; Homans' (1961:75) concern with guilt or anger when the principle of "distributive justice" is violated in social exchange; Gross and Stone's (1964) empirical investigation of embarrassment; Goffman's (1967) analysis of "face" and shame in interaction; Garfinkel's (1967:50-51) conclusions concerning anger and embarrassment when interaction takes an untoward course. Not surprisingly, sociologists discuss emotions most often within the context or as the results of particular social conditions—for example, types of division of labor or of social relations. These are the social cues or reinforcement contingencies that evoke emotions. Therefore, a systematic sociological theory of emotions requires a sociological model of the conditions that may vary so as to induce emotions. I believe there is empirical support for a certain model of social relations which offers a basis for a sociological theory of emotions. I turn to this now.

A Model of Social Relations

The most important premise of any sociological theory of emotions must be that *an extremely large class of human emotions results from real, anticipated, imagined, or recollected outcomes of social relationships*; she says she does not love me; he says I did a good job; I claimed to be honest, but was caught in a lie; he obligated himself to me, but then reneged; and so forth. These are outcomes of social relationships that ought to stimulate emotion. It follows that we would understand the production of emotions better if we understood social relationships better. I shall suggest now a taxonomy of social relationships which can constitute a systematic specification of the situational matrix which produces emotion.

Many recent studies have sought to identify the basic dimensions of relational behavior or personality in the social setting (many studies in this literature are examined in Kemper [1973; in press], including Carter [1954], Schutz [1958], Lorr and McNair [1963], Borgatta [1964], Longabaugh [1966], and Benjamin [1974]). The usual technique is to factor analyze a set of scores of behavioral or personality items. While there is relatively little agreement on the total number of dimensions of social behavior or personality in the social context, two clearly relational factors have always emerged.

Despite different names and nuances assigned to the factors, the items point to two underlying relational themes: *control of one actor by another* and *degree of positive social relations*. Elsewhere (Kemper, 1972, 1973, 1974), I have labeled these two dimensions *power* and the giving of *status*.

Generally, the *power* dimension of relationship encompasses such acts as coercing, forcing, threatening, punishing, dominating, and so on. This relational understanding of power is close to Weber's notion of 'overcoming the resistance' of the other (Weber, 1946:180). It should be apparent that actors do not *ordinarily* use power unless the other resists giving what is wanted or could conceiva-

bly resist in the future. What is wanted, of course, are various benefits, rewards, and privileges. Power is a dimension of relationship in which, as Weber (1946) suggests, benefits are obtained from others who do not confer them willingly.

The *status* dimension of relationship, on the other hand, accounts for the voluntary, uncoerced giving of benefits, rewards, and privileges. Thus, one actor willingly complies with, approves of, gives money, praises, emotional support, friendship, or even love, to an other because that actor wants to do so. The benefits and rewards are freely offered without the use of power.

Because of the large number of studies which have reported the power and status dimensions as central to relational behavior, I shall use these dimensions to generate a set of relational structures and outcomes that might produce certain distressful emotions.

Toward A Sociological Theory of Distressful Emotions

Each actor (in a dyadic setting) has a varying amount of power over the other.

Each actor also receives a varying amount of status from the other. An actor can sense or feel that he has, or has used, an *excess* of power in his relations with the other, or that he has acquired or claimed an *excess* of status. An actor can also sense or feel that he has *insufficient* power or receives *insufficient* status from the other. These make a set of four relational possibilities from the perspective of the given actor (see Figure). The actor may feel excess or insufficiency of power and/or status as a result of comparing his actions with internalized social norms, idiosyncratic sentiments acquired in the course of a lifetime, persuasive labeling by others (Scheff, 1963), misperception or distortion of communications from others, and so on.

I introduce now the concept of *agency*, which entails the actor's sense of who is responsible for the excess or insufficiency of power and/or status (cf. Pastore, 1952). I propose that when *self* is viewed as agent, the emotion will be *introjected* and *intropunitive*. When *other* is viewed as agent, the emotion will be *extrojected* and *extropunitive*. When the actor's sense of agency and responsibility oscillates be-

FIGURE

SOCIAL RELATIONAL MATRIX OF DISTRESSFUL EMOTIONS

	POWER	STATUS
Agent: <u>Self</u> FELT EXCESS	GUILT Remorse; desire for punishment; expiation Megalomania; blame the victim	SHAME Withdrawal or desire to recompense Hypercriticism and perfectionism
Agent: <u>Other</u>	Doom, dread, fear Anarchic-rebelliousness	Despair, apathy, sense of worthlessness Anger and hostility

tween self and other, the felt relational excess or deficit will produce *compound*, or *mixed* emotions.

Taking into account the several conceptual distinctions presented—felt excess or insufficiency of power and/or status, and self-or-other as responsible agent—I propose particular emotions for each of the relational conditions. These are shown in the figure and I shall discuss them in turn.²

(1) *Guilt*: When an individual senses that he has used excessive power against another he will feel the emotion of guilt. Ausubel (1955) defines guilt as "a special kind of negative self-evaluation which occurs when an individual acknowledges that his behavior is at variance with a given moral value to which he feels obligated to conform" (1955:379). Moral values overwhelmingly refer to relational conduct: the Decalogue, the Sermon on the Mount, the teachings of the Buddha. Transgressions of moral values—murder, assault, theft, lies, etc.—can be seen as violations of standards relating to the use of *power* in social relationships.

When *self* is seen as the responsible agent, the guilt feelings are introjected and the emotion of guilt will be felt as regret, remorse, and a desire for relief of the psychic pain, possibly through some form of *expiation* (cf. Galtung, 1958). The clinical literature is filled with cases in which feelings of guilt are introjected and patients express the need to be punished for their real or imagined transgressions (Sarnoff, 1962; Lewis, 1971).

If, on the other hand, guilt feelings are extrojected through viewing the *other* as the responsible agent, the punitive orientation is turned away from the self and the result will be a form of *megalomania*. This is a more or less delusionary view of the self in grandiose terms, based principally on the sense of self as a mighty power who has the right to determine the fates of others—even, apparently, whether they

will live or die. Megalomania mediates guilt by casting the responsibility for the use of excess power upon the other, thus blaming the victim (Ryan, 1971). But the act of blaming the victim and the self-aggrandizement that goes with it are responses to having used excess power in the first place, and are modes of handling the discomfort of guilt. They are responses of guilt just as is the conventional introjected mode where the discomfort is mediated by acceptance of punishment.³

(2) *Shame*: Shame is the emotion experienced when an actor believes that he has claimed and/or received more status than he deserves. Status, as defined above, is the amount of reward, recognition, and gratification which others give to the actor *voluntarily*, without his coercion. Status is ordinarily given for competence or achievement in the division of labor, or for competence in social relationships. Thus, when an actor claims more competence than he has, or is offered and accepts more status than he deserves, he feels shame.

Even momentary lapses of competence make us subject to shame, although it is usually called embarrassment (Ausubel, 1955; Weinberg, 1968; Modigliani, 1968). Whether momentary or longlasting, the experience is one of "self-depreciation vis-a-vis the group" (Ausubel, 1955:382) or "loss of self-esteem" (Modigliani, 1968:315).

When the actor views *self* as the agent of the failed status-claim, the shame will be introjected, and there will either be a

² The emotions treated here as consequences of certain felt patterns of power-status outcomes are distressful or "negative." Positive emotions—such as security and happiness—are also consequences of felt outcomes of power-status relations (see Kemper, in press, for discussion of these).

³ Although the four cells of Figure One are designated by ordinary emotional labels—guilt, shame, etc.—the introjected and extrojected portions of each cell are variously moods, coping behaviors, character traits, and so on. This points up the oft-noted lack of a mutually exclusive set of labels of a single modality which can be mapped onto the autonomic-motoric-cognitive states that comprise emotions (cf. Young, 1961:352–353; Osgood, 1966:29; Lazarus and Averill, 1972:244). Another aspect of this problem concerns the differential naming of different points of intensity of single emotions: for example, concern, apprehension, fear, anxiety, dread—each a different level of intensity of one emotion. I have chosen emotional labels according to the levels of intensity conventionally discussed in the literature. Intensity of emotion is largely a function of the felt intensity of the outcomes of power-status relations.

withdrawal from interaction—the “re-treatist” alternative (Merton, 1957)—or a desire to *recompense* those who were induced to give status under false pretenses. Thus, the embezzler promises to repay, the coward places himself in the most perilous position, each in order to revalidate his status.

Alternatively, when the *other* is seen as agent, the shame is extrojected. This occurs often when the other is the agent of exposure of the status discrepancy, as in a joke told as the actor's expense, or the revelation of a peccadillo. Extropunitive versions of shame turn the searchlight for incompetence on others. This gives rise, I suggest, to a *hypercritical-perfectionism* directed toward the other which reequilibrates the status system, though at a lower level for both self and other. In effect, the actor says, “If I did not deserve the status I received, neither do you!” Thibaut and Kelly (1959:234) see this mode of status reduction as a general solution for persons of low status: “. . . we would expect him [the low status person] to try to improve his level of outcomes or, what could have the same effect, to reduce the outcomes of those better off than he is.” Of course, the latter alternative would appeal to those who feel others are responsible for their loss of face.

In ordinary discourse there is frequent confusion between guilt and shame, and some authors even question whether there is a difference between the two (e.g., Bandura and Walters, 1963). The relational perspective used here maintains the analytical and empirical distinctiveness of the two emotions (cf. Lynd, 1958:20–26).

(3) *Anxiety*: When there is an imbalance in the power relationship between actors, the one with relatively less power is vulnerable to the encroachments of the other, and the anticipation that other will use power is the core of anxiety. Whether or not the anxiety is of pathological intensity and related to repressed experiences, the relational context is one of power relations. Weems and Wolowitz (1969:191) have found that “self-perceived power deficit is a demonstrably prominent factor in the dynamics of . . . paranoids,” for whom anxiety is a principal symptom. Lemert's (1962) description of the devel-

opment of paranoia also reveals how the social environment actually exerts considerable power behavior—acting in secret, isolating the actor, excluding him, and so forth—which fosters in the actor the sense that others are acting against him.

The role of power in the development of anxiety was recognized by Freud (1966:394):

On what occasions anxiety appears . . . will of course depend to a large extent on the state of the person's knowledge and on his sense of *power vis-a-vis* the external world. (emphasis added)

The important “external world” for most human actors is, of course, the social world, and power is held in relation to other actors.

When *self* is seen as responsible for the power deficit, the actor has insufficient power to deter the other and focuses on his own incapacities and deficiencies. Anxiety will be introjected and the power deficit will bring about the classic sense of dread and impending doom often associated with anxiety (cf. Portnoy, 1959:308). The actor feels helpless to prevent the oncoming blow.

When *other* is seen as responsible for the power deficit, the anxiety is extrojected and the response takes a different form. The actor focuses on the other as agent and assigns the other the intent and will to overcome him and benefit thereby. This leads to resistance and an effort to destroy the other's power or the bases of that power—a form of *rebelliousness* or *anarchy*. Brehm (1966) indicates a strong counter-power reaction to an other's efforts to control the actor's previously “free” movements. Stokols (1975) also differentiates between “subjugation” (introjected form) and “rebellion” (extrojected form) as responses to power imbalances in favor of the other.

(4) *Depression*: Depression results from a deficit of status, i.e., an insufficiency of reward and gratification given voluntarily by others. This may be due to rejection by a loved one, loss of a loved one, rejection of one's work, etc. Each of these losses or rejections has the implication that the expected, hoped for, or customary gratifications which the other(s) could provide are

not, or will not be forthcoming (cf. Lazarus, 1968; Gewirtz, 1969).

In the intropunitive type of depression, where *self* is seen as the agent of the status deficit, there is the classic syndrome of withdrawal, *despair*, apathy, feelings of worthlessness, and, at the extreme, suicide.

In the extropunitive response to insufficient status, where *other* is seen as the agent, anger and hostility are directed against the other, and often generalized against all possible others who may potentially have status to withhold. In the apathy-despair response in depression there is a sensed justice in the judgment of others; in the hostile response, their judgment to withhold status is rejected as unjust.

The relational view of depression differs from the Freudian view by treating depression not as a result of hostility turned inward, but as a result of deprivation of gratifications received from others. Depression is simply hunger for what others have to offer and have not given, or have withdrawn.

* * *

I have proposed that guilt, shame, anxiety, depression, and anger stem from certain felt excesses or deficiencies in either power or status, in conjunction with judgments concerning agency.⁴ It is also obvious that there are connections between these distressful emotions. Fairbairn (1952), a psychiatrist, reports that as the schizophrenic patient's ego begins to reintegrate, the patient shifts from a paranoid-schizoid position into depression. In the relational terms sketched here, this implies a shift from problems of *power* to problems of *status*, from the problem of how to avoid destruction to the problem of how to acquire gratification.

Fairbairn's point, however, encourages a far-reaching surmise, namely that, very broadly speaking, the disorders labeled schizophrenia are essentially disorders associated with power relations and the

disorders involving depression are disorders of status. This is not to say that power disorders and status disorders are mutually exclusive. In fact, they readily interpenetrate, not only because social relationships are comprised of both power and status dimensions, but also because when there is a breakdown in one relational dimension, it is likely the other dimension will suffer as well. If one feels guilt, it is easy also to feel shame (due to a failure to validate the virtuous self presented to others), anxiety (due to fear of retribution for the initial trespass which inspired the guilt) and depression (due to real or imagined status-loss which may be the punishment for the transgression).

It should be obvious that these conclusions concerning guilt, shame, anxiety, depression, and anger, and the relational roots of the two major categories of psychoses—schizophrenia and depressive disorders—are possible only from a sociological stance: emotions flow from (felt) real, anticipated, imagined, or recollected outcomes of *social relations*. It should also be apparent that the sociologist must have some model of social relations with which to explicate or predict emotions. I have proposed a model involving the two dimensions of power and status. Other researchers may prefer a different nomenclature for the underlying concepts, but I believe that the power and status concepts are so fundamental for the analysis of emotions that they will be retained by whatever name.

I turn now to an important psychophysiological problem in the study of emotions which quite unexpectedly was clarified when the power-status relational dimensions were applied. Even more unexpectedly, the solution lent persuasive support to the theoretical soundness of the relational dimensions themselves.

Toward A Sociophysiology of Emotions

Emotions are physiologically rooted in the organism. Ordinarily this would turn sociological attention away from the phenomena, for sociologists seldom "do sociology" and know physiology too. Yet

⁴ I have not identified all possible sources of the five distressful emotions, but have indicated the social relational outcomes that would cue them.

I want to report a startling and welcome result that gives support to the power-status formulation at the sociological level while it clarifies a longstanding problem at the psychophysiological level. There is too little space available to cover the question extensively, but I shall summarize the basic facts (see Kemper, in press, for an extensive discussion of these materials).

The autonomic basis for emotions is in the *sympathetic* and the *parasympathetic* branches of the nervous system. These operate together to maintain a *more or less* homeostatic state along the emotional front. The sympathetic system is generally associated with emotions of arousal such as anger and fear, while the parasympathetic is generally associated with emotions of pleasure and satisfaction. Each system is connected with the peripheral organs—heart, stomach, blood vessels, lungs, pupils, etc.—that contribute the physical signs of emotion. Each system operates more or less to modulate the other when it reaches a level of activation that threatens either to “blow up” the organism from over-arousal or to “wind it down” from excess of calm; responseless, contentment (see Lex, 1974, for a short cogent discussion of the interaction of the two systems in the case of “voodoo” death).

A very considerable body of research (Funkenstein, 1955, reports the fundamental developments) supports the following hypothesis: when an individual experiences what is generally agreed to be the emotion of anger, a particular neurochemical called norepinephrine (or noradrenaline) is released in the body. On the other hand, when the individual experiences what is commonly understood to be fear or anxiety, a hormone called epinephrine (or, more commonly, adrenaline) is released. Of the utmost importance for this discussion is that *no other hormones or neurochemicals have been found to be so specifically related to particular emotions*. This extremely important body of experimental research, developed in the 1950s and early 1960s, permits the following heuristic conclusion.

Since deficit of own power (or excess of other's power) is the social relational condition for fear or anxiety, and since loss of

customary, expected, or deserved status (other as agent) is the basic relational condition for anger, it seems entirely compatible to suppose that specific social relational conditions are accompanied by specific physiological reactions, with the felt emotions as the psychological mediators between the two. Furthermore, the fact that only two neurochemical substances appear to differentiate the power emotion (fear) from the status-loss emotion (anger), suggests that two may be a correct estimate of the number of separate dimensions of relationship. Though it may be only coincidental to find two social relational dimensions and two neurochemicals of specific emotions, there is much to be gained from viewing the two findings as mutually supportive theoretically as well as concretely in the occurrence of emotions. It makes sense, I believe (and a close analysis of the problem of emotions demands it), that there be some kind of integration between the social relations we enact and the cognitive-somatic-autonomic accompaniments of the interaction. If there is not, there is no sense to studies of “social stress,” or to the social facet of psychosomatic medicine.

Yet it isn't quite this simple. I will take up two additional considerations. First, I have said nothing about positive emotions, which seem to have something to do with the parasympathetic nervous system where neither norepinephrine nor epinephrine are found. Indeed, the major neurochemical in the parasympathetic system is *acetylcholine*. This substance seems to be involved with consummatory responses, contentment, mild satisfaction, and a sense of well-being. *Relationally* that would mean that felt levels of *both* power and status are *adequate*—neither excessive, producing either guilt or shame; nor insufficient, producing either fear or anger. Thus, the conclusion now reads: the parasympathetic nervous system dominates when both power and status are sufficient and no specially compelling emotions are felt; the sympathetic nervous system dominates when power and/or status are disturbed from satisfactory levels, with norepinephrine as the organismic correlate of status loss (anger) and epinephrine as the organismic corre-

late of power-insufficiency (fear).⁵ Thus, the additional consideration of relatively calm positive feelings does not disturb the initial formulation, in which specific neurochemical substances correspond to specific social relational conditions.

A second consideration in regard to the sociophysiological integration I have proposed is that the specificity theory in psychophysiology, first proposed by William James (1893), has many opponents. They deny the specificity of physiological response that I have detailed above (Cannon, 1929). They claim that a common physiological substrate underlies all emotions and that the differentiation of emotions is based on other grounds. Schachter and Singer (1962) present a particularly persuasive formulation for sociologists. They argue that the individual looks to the social environment to see "what is happening," so to speak, engaging in "social comparison" as defined by Festinger (1954). In a quite ingenious experiment that has become a classic, Schachter and Singer (1962) appeared to establish that emotions as different as anger and euphoria can both be associated with epinephrine, the substance which had been found in previous research to be associated only with fear. Thus, with one seemingly "crucial" experiment Schachter and Singer appeared to demolish the entire structure of specificity theory, and the specificity viewpoint is hardly pursued today in psychophysiology (Levi, 1972, and Frankenhaeuser, 1971, also report research that seems to support the antispecificity perspective).

But there is one problem with this antispecificity research: a *sociological* analysis of the experimental conditions in each of the studies reveals that, where measured, norepinephrine flowed when relational conditions reflected status-withdrawal and, by inference, anger; and that, where measured, epinephrine was secreted when relational conditions reflected insufficient power of self, or ex-

cess power of other and, by inference, fear or anxiety. Where the neurochemical secretions themselves were not measured, a straightforward analysis of the power-status *relational structure* of the experimental design reveals that relational conditions appropriate to evoke emotions of fear or anger, or their common behavioral manifestations, were present. The anti-specificity experimenters are aware in only the most vague way of the relational nature of their experimental designs, thereby completely ignoring the quite obvious evidence for specificity in their own data. Ironically, since those psychophysiologicalists who espouse specificity theory are also not sociologists, they too have failed to see that the sociological structure of their opponents' experiments invalidated the conclusions drawn from them.

From a strictly sociological viewpoint, it does not matter whether specificity theory is correct or not. Sociologists must still detail the range and extent of systematic variability of social conditions that are associated with the production of emotions. But there is an importance and an elegance to the integration of sociological relational theory with psychophysiological specificity theory that warrants its vigorous investigation. Sociology made an unexpected eleventh-hour save of specificity theory. At the same time, the psychophysiological evidence for specificity lends unexpected support to sociology's two relational dimensions. Psychophysiologicalists—with the assistance of sociologists who are sensitive to the social relational aspects of experimental paradigms—must devise new experiments to test specificity and to establish it now more positively. This work must proceed on the basis of an informed integration of three disciplines: physiology, psychology, and sociology. No one or two of them can solve the problem alone.

Conclusion

In an extremely brief space I have tried to present an overview of the field of emotions as currently practiced by psychologists, as well as some sociological contributions to the study of emotions. This by

⁵ Gellhorn and Louffbourrow (1963) and Gellhorn (1967) are important interpreters of the "interaction effects" of the sympathetic and parasympathetic systems in the production of emotions.

no means exhausts the domain of sociological interest in the field. Indeed, the emotions—as the basis of motivation and wide spans of human action—are ever present, though they may be repressed or suppressed, or institutionalized away (as in Weber's "disenchantment of the world" by the ethos of rationalization). Emotions maintain, alter, and sometimes destroy many social processes: marriage and family relations; relationships across authority levels in work settings (Myers, 1977); competition and conflict between social groups such as classes, ethnic groups, or athletic teams; collective behavior phenomena, whether a reported invasion from Mars (Cantril, 1940), racial conflict (Ransford, 1968), a stock market panic (Weber, 1947:92), or economic boom or bust (Durkheim, 1951; Bensman and Vidich, 1962). In a very suggestive report, Collins (1975) proposes that group emotions are manipulated by social hierarchies so as to legitimate the distribution of benefits and control in the social order. Sociological analysis of emotions, so long in emerging as a systematic study, may yet establish sociology more firmly both as science and as informed interpreter of social life.

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ENVIRONMENTAL SOCIOLOGY: A NEW PARADIGM*

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Ostensibly diverse and competing theoretical perspectives in sociology are alike in their shared anthropocentrism. From any of these perspectives, therefore, much contemporary and future social experience has to seem anomalous. Environmental sociologists attempt to understand recent societal changes by means of a nonanthropocentric paradigm. Because ecosystem constraints now pose serious problems both for human societies and for sociology, three assumptions quite different from the prevalent Human Exceptionalism Paradigm (HEP) have become essential. They form a New Environmental Paradigm (NEP). Sociologists who accept this New Environmental Paradigm have no difficulty appreciating the sociological relevance of variables traditionally excluded from sociology. The core of environmental sociology is, in fact, study of interactions between environment and society. Recent work by NEP-oriented sociologists on issues pertaining to social stratification exemplifies the utility of this paradigm.

Sociology appears to have reached an impasse. Efforts of sociologists to assimilate into their favorite theories some of the astounding events that have shaped human societies within the last generation

have sometimes contributed more to the fragmentation of the sociological community than to the convincing explanation of social facts. But as Thomas Kuhn (1962:76) has shown, such an impasse

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often signifies "that an occasion for re-tooling has arrived."

The rise of environmental problems, and especially apprehensions about "limits to growth," signalled sharp departures from the exuberant expectations most sociologists had shared with the general public. Environmental problems and constraints contributed to the general uneasiness in American society brought about by events in the sixties. Sociologists, no less than other thinking people, are still grappling with the dramatic shift from the calmer fifties, when the American dreams of social progress, upward mobility, and societal stability seemed secure.

In 1976 the American Sociological Association, following precedents set a few years earlier in the Rural Sociological Society and in the Society for the Study of Social Problems, established a new "Section on Environmental Sociology."¹ In this paper we shall try to account for the development of environmental sociology by showing how it represents an attempt to understand recent societal changes that are difficult to comprehend from traditional sociological perspectives. We contend that, rather than simply representing the rise of another speciality within the discipline, the emergence of environmental sociology reflects the development of a new paradigm, and that this paradigm may help to extricate us from the impasse referred to above.

The "New Environmental Paradigm" (NEP) implicit in environmental sociology is, of course, only one among several current candidates to replace or amend the increasingly obsolescent set of "domain assumptions" which have defined the nature of social reality for most sociologists. Environmental sociologists, no less than the advocates of the very different alternatives Gouldner (1970) has described, are attempting to come to grips with a changed "sense of what is real." Further, we believe the NEP may contribute to a better understanding of contemporary and future social conditions than is possible with previous sociological perspectives. To illustrate the power of this paradigm to

shed new light on important sociological issues, we shall briefly describe some recent NEP-based examinations of problems in stratification. But first we must contrast the old and new sets of assumptions.

The "Human Exceptionalism Paradigm"

The numerous competing theoretical perspectives in contemporary sociology—e.g., functionalism, symbolic interactionism, ethnomethodology, conflict theory, Marxism, and so forth—are prone to exaggerate their differences from each other. They purport to be paradigms in their own right, and are often taken as such (see, e.g., Denisoff, *et al.*, 1974, and Ritzer, 1975). But they have also been construed simply as competing "pre-paradigmatic" perspectives (Friedrichs, 1972). We maintain that their apparent diversity is not as important as the fundamental anthropocentrism underlying *all* of them.

This mutual anthropocentrism is part of a basic sociological worldview (Klausner, 1971:10–11). We call *that* worldview the "Human Exceptionalism Paradigm" (HEP). We contend that acceptance of the assumptions of the HEP has made it difficult for most sociologists, regardless of their preferred orientation, to deal meaningfully with the social implications of ecological problems and constraints. Thus, the HEP has become increasingly obstructive of sociological efforts to comprehend contemporary and future social experience.

The HEP comprises several assumptions that have either been challenged by recent additions to knowledge, or have had their optimistic implications contradicted by events of the seventies. Accepted explicitly or implicitly by all existing theoretical persuasions, they include:

1. Humans are unique among the earth's creatures, for they have culture.
2. Culture can vary almost infinitely and can change much more rapidly than biological traits.
3. Thus, many human differences are socially induced rather than inborn, they can be socially altered, and in-

¹ In the late sixties a Natural Resources Research Group was formed in the RSS, and in 1973 SSSP established an Environmental Problems Division.

convenient differences can be eliminated.

4. Thus, also, cultural accumulation means that progress can continue without limit, making all social problems ultimately soluble.

Sociological acceptance of such an optimistic worldview was no doubt fostered by prevalence of the doctrine of progress in Western culture, where academic sociology was spawned and nurtured. It was under the American branch of Western culture that sociology flourished most fully, and it has been clear to foreign analysts of American life, from Tocqueville to Laski, that most Americans (until recently) ardently believed that the present was better than the past and the future would improve upon the present. Sociologists could easily share that conviction when natural resources were still so plentiful that limits to progress remained unseen. The historian, David Potter (1954:141), tried to alert his colleagues to some of the unstated and unexamined assumptions shaping their studies; his words have equal relevance for sociologists: "The factor of abundance, which we first discovered as an environmental condition and which we then converted by technological change into a cultural as well as a physical force, has . . . influenced all aspects of American life in a fundamental way."²

Not only have sociologists been too unmindful of the fact that our society derived special qualities from past abundance; the heritage of abundance has made it difficult for most sociologists to perceive the possibility of an era of uncontrived scarcity. For example, ecological concepts such as "carrying capacity" are alien to the vocabularies of most sociologists (Catton, 1976a; 1976b), yet disregard for this concept has been tantamount to assuming an environment's carrying capacity is always enlargeable as needed—thus denying the possibility of scarcity.

Neglect of the ecosystem-dependence

² For an early warning that this exuberance-producing force could be temporary, see Sumner (1896). Few twentieth century sociologists have taken the warning seriously.

of human society has been evident in sociological literature on economic development (e.g., Horowitz, 1972), which has simply not recognized biogeochemical limits to material progress. And renewed sociological attention to a theory of societal evolution (e.g., Parsons, 1977) has seldom paid much attention to the resource base that is subjected to "more efficient" exploitation as societies become more differentiated internally and are thereby "adaptively upgraded." In such literature, the word "environment" refers almost entirely to a society's "symbolic environment" (cultural systems) or "social environment" (environing social systems).³

It is the habit of neglecting laws of other sciences (such as the Principle of Entropy and the Law of Conservation of Energy)⁴—as if human actions were unaffected by them—that enables so distinguished a sociologist as Daniel Bell (1973: 465) to assert that the question before humanity is "not subsistence but standard of living, not biology, but sociology," to insist that basic needs "are satiable, and the possibility of abundance is real," to impute "apocalyptic hysteria" to "the ecology movement," and to regard it as trite rather than questionable to expect "compound interest" growth to continue for another hundred years. Likewise, this neglect permits Amos Hawley (1975:8–9) to write that "there are no known limits to the improvement of technology" and the population pressure on nonagricultural resources is neither "currently being felt or likely to be felt in the early future." Such views reflect a staunch commitment to the HEP.

Environmental Sociology and the "New Environmental Paradigm"

When public apprehension began to be aroused concerning newly visible environmental problems, the scientists

³ Even sociological human ecologists have limited their attention primarily to the *social* or *spatial* environment, rather than the *physical* environment (see Michelson, 1976:17–23), reflecting their adherence to the HEP.

⁴ See Miller (1972) for a lucid discussion of these laws.

who functioned as opinion leaders were not sociologists. They included such individuals as Rachel Carson, Barry Commoner, Paul Ehrlich and Garrett Hardin—biologists. Leadership in highlighting the precariousness of the human condition was mostly forfeited by sociologists, because until recently, most of us had been socialized into a worldview that makes it difficult to recognize the reality and full significance of the environmental problems and constraints we now confront. Due to our acceptance of the HEP, our discipline has focused on humans to the neglect of habitat; consideration of our *social* environment has crowded out consideration of our physical circumstances (Michelson, 1976:17). Further, we have had unreserved faith that equilibrium between population and resources could and would be reached in noncatastrophic ways, since technology and organization would mediate the relations between a growing population and its earthly habitat (see, e.g., Hawley, 1975).

But, stimulated by troubling events, some sociologists began to read such works as Carson (1962), Commoner (1971), Ehrlich and Ehrlich (1970), and Hardin (1968), and began to shed the blinders of the HEP. As long-held assumptions began to lose their power over our perceptions, we began to recognize that the reality of ecological constraints posed serious problems for human societies and for the discipline of sociology (see, e.g., Burch, 1971). It began to appear that, in order to make sense of the world, it was necessary to rethink the traditional Durkheimian norm of sociological purity—i.e., that social facts can be explained *only* by linking them to other *social* facts. The gradual result of such rethinking has been the development of environmental sociology.

Environmental sociology is clearly still in its formative years. At the turn of the decade rising concern with "environment" as a social problem led to numerous studies of public attitudes toward environmental issues and of the "Environmental Movement" (see Albrecht and Mauss, 1975). A coalition gradually developed between sociologists

with such interests and sociologists with a range of other concerns—including rather established interests such as the "built" environment, natural hazards, resource management and outdoor recreation, as well as newer interests such as "social impact assessment" (mandated by the National Environmental Policy Act of 1969). After the energy crisis of 1973, numerous sociologists (including many with prior interests in one or more of the above areas) began to investigate the effects of energy shortages in particular, and resource constraints in general, on society: the stratification system, the political order, the family, and so on. (For an indication of the range of interests held by environmental sociologists see Dunlap, 1975, and Manderscheid, 1977; for reviews of the literature see Dunlap and Catton, forthcoming, and Humphrey and Buttel, 1976.)

These diverse interests are linked into an increasingly distinguishable specialty known as environmental sociology by the acceptance of "environmental" variables as meaningful for sociological investigation. Conceptions of "environment" range from the "manmade" (or "built") environment to the "natural" environment, with an array of "human-altered" environments—e.g., air, water, noise and visual pollution—in between. In fact, *the study of interaction between the environment and society is the core of environmental sociology*, as advocated several years ago by Schnaiberg (1972).⁵ This involves studying the effects of the environment on society (e.g., resource abundance or scarcity on stratification) and the effects of society on the environment (e.g., the contributions of differing economic systems to environmental degradation).⁶

The study of such interaction rests on the recognition that sociologists can no longer afford to ignore the environment in

⁵ This does not mean that environmental sociologists focus only on bi-variate relationships between social and environmental variables, as illustrated by Schnaiberg's (1975) "societal-environmental dialectic" (to be discussed below).

⁶ For an alternative and narrower view of the domain of environmental sociology see Zeisel (1975: Chap. 1).

their investigations, and this in turn appears to depend on at least tacit acceptance of a set of assumptions quite different from those of the HEP. From the writings of several environmental sociologists (e.g., Anderson, 1976; Burch, 1971, 1976; Buttél, 1976; Catton, 1976a, 1976b; Morrison, 1976; Schnaiberg, 1972, 1975) it is possible to extract a set of assumptions about the nature of social reality which stand in stark contrast to the HEP. We call this set of assumptions the "New Environmental Paradigm" or NEP (see Dunlap and Van Liere, 1977 for a broader usage of the term, referring to emerging public beliefs):

1. Human beings are but one species among the many that are interdependently involved in the biotic communities that shape our social life.
2. Intricate linkages of cause and effect and feedback in the web of nature produce many unintended consequences from purposive human action.
3. The world is finite, so there are potent physical and biological limits constraining economic growth, social progress, and other societal phenomena.

Environmental Facts and Social Facts

Sociologists who adhere to the NEP readily accept as factual the opening sentences of the lead article (by a perceptive economist) in a recent issue of *Social Science Quarterly* devoted to "Society and Scarcity": "We have inherited, occupy, and will bequeath a world of scarcity: resources are not adequate to provide all of everything we want. It is a world, therefore, of limitations, constraints, and conflict, requiring the bearing of costs and calling for communal coordination" (Allen, 1976:263). Persistent adherents of the HEP, on the other hand, accustomed to relying on endless and generally benign technological and organizational breakthroughs, could be expected to discount such a statement as a mere manifestation of the naive presumption that the "state of the arts" is fixed (see, e.g., Hawley, 1975:6-7).

Likewise, sociologists who have been

converted to the assumptions of the NEP have no difficulty appreciating the sociological relevance of the following fact: the \$36 billion it now costs annually to import oil to supplement depleted American supplies is partially defrayed by exporting \$23 billion worth of agricultural products—grown at the cost of enormous soil erosion (van Bavel, 1977). Environmental sociologists expect momentous social change if soil or oil, or both are depleted. But sociologists still bound by the HEP would probably ignore such topics, holding that oil and soil are irrelevant variables for sociologists. However, we believe that only by taking into account such factors as declining energy resources can sociologists continue to understand and explain "social facts." We will attempt to demonstrate this by examining some work by NEP-oriented sociologists in one of the areas they have begun to examine—social stratification.

Usefulness of the NEP: Recent Work in Social Stratification

The bulk of existing work in stratification appears to rest on the Human Exceptionalism Paradigm, as it "... does not adequately consider the context of resource constraints or lack thereof in which the stratification system operates" (Morrison, 1973:83). We will therefore describe recent work in the area by environmental sociologists, in an effort to illustrate the insights into stratification processes provided by the NEP. We will limit the discussion to three topics: the current decline in living conditions experienced by many Americans; contemporary and likely future cleavages in our stratification system; and the problematic prospects for ending self-perpetuating poverty.

Recent decline in standard of living: A majority of Americans are concerned about their economic situation (Strumpel, 1976:23), and in *Food, Shelter and the American Dream*, Aronowitz (1974) exemplifies the growing awareness that *something* is not going according to expectation—that old ideals of societal progress, increasing prosperity and material comfort, and individual and inter-

generational mobility for *all* segments of society are *not* being realized (also see Anderson, 1976:1-3). Yet, even a "critical sociologist" such as Aronowitz seems impeded by the HEP in attempting to understand these changes. He views recent shortages in food, gasoline, heating oil, and so on, entirely as the result of "manipulations" by large national and supranational corporations, and is skeptical of the idea that resource scarcities may be real. Thus, his solution to the decline in the American standard of living would apparently be solely political—reduce the power of large corporations.

Although many environmental sociologists would not deny that oil companies have benefited from energy shortages, their acceptance of the NEP leads to a different explanation of recent economic trends. Schnaiberg (1975:6-8), for instance, has explicated a very useful "societal-environmental dialectic." Given the *thesis* that "economic expansion is a social desideratum" and the *antithesis* that "ecological disruption is a necessary consequence of economic expansion," a dialectic emerges with the acceptance of the proposition that "ecological disruption is harmful to human society." Schnaiberg notes three alternative *syntheses* of the dialectic: (1) an *economic synthesis* which ignores ecological disruptions and attempts to maximize growth; (2) a *managed scarcity synthesis*⁷ which deals with the most obvious and pernicious consequences of resource-utilization by imposing controls over selected industries and resources; and (3) an *ecological synthesis* in which "substantial control over both production and effective demand for goods" is used to minimize ecological disruptions and maintain a "sustained yield" of resources. Schnaiberg (1975:9-10) argues that the synthesis adopted will be influenced by the basic economic structure of a society, with "regressive" (inequality-magnifying)

societies most likely to maintain the "economic" synthesis and "progressive" (equality-fostering) societies least resistive to the "ecological" synthesis.⁸ Not surprisingly, therefore, the U.S., with its "non-redistributive" economy, has increasingly opted for "managed scarcity" as the solution to environmental and resource problems.⁹

Managed scarcity involves, for example, combating ecological disruptions by forcing industries to abate pollution, with resultant costs passed along to consumers via higher prices, and combating resource shortages via higher taxes (and thus higher consumer prices) on the scarce resources. There is growing recognition of the highly regressive impacts of both mechanisms (Morrison, 1977; Schnaiberg, 1975), and thus governmental reliance on "managed scarcity" to cope with pollution and resource shortages at least partly accounts for the worsening economic plight of the middle-, working, and especially lower-classes—a plight in which adequate food and shelter are often difficult to obtain. Unfortunately, these economic woes cannot simply be corrected by returning to the economic synthesis. The serious health threats posed by pollutants, the potentially devastating changes in the ecosystem wrought by unbridled economic and technological growth (e.g., destruction of the protective ozone layer, alteration of atmospheric temperature), and the undeniable reality of impending shortages in crucial resources such as oil, all make reversion to the traditional economic synthesis impossible in the long run (see, e.g., Anderson, 1976; Miller, 1972). Of course, as Morrison (1976) has noted, the pressures to return to this synthesis are great, and understanding them

⁸ Thus, e.g., among industrialized nations Sweden appears to have come closest to the ecological synthesis, while China represents the closest approximation to it by any developing nation (Anderson, 1976:242-251). In contrast, highly regressive developing nations such as Brazil seem strongly committed to the economic synthesis.

⁹ The U.S. economy is "non-redistributive" because overall patterns of inequality (i.e., relative shares of wealth) have been altered very little by growth, even though all strata have improved their lot via growth (Schnaiberg, 1975:9; Zeitlin, 1977: Part 2).

⁷ Schnaiberg's term was "planned scarcity," but because adherents of the HEP might suppose that phrase referred to scarcity *caused* by planners—rather than scarcities (and their costs) *allocated* by planners—we prefer to speak of "managed scarcity."

provides insights into contemporary and future economic cleavages.

Cleavages within the stratification system: Schnaiberg's ecological synthesis amounts to what others have termed a "stationary" or "steady-state" society, and it is widely agreed that such a society would need to be far more egalitarian than the contemporary U.S. (Anderson, 1976:58-61; Daly, 1973:168-170).¹⁰ Achieving the necessary redistribution would be very difficult, and opposition to it would be likely to result in serious, but unstable cleavages within the stratification system. In the long run, as environmental constraints become more obvious, ecologically aware "haves" are likely to opt for increased emphasis on managed scarcity to cope with them. The results would be disastrous for the "have nots," as slowed growth and higher prices would reverse the traditional trend in the U.S. in which *all* segments of society have improved their material condition—not because they obtained a larger slice of the "pie," but because the pie kept growing (Anderson, 1976:28-33; Morrison, 1976). Slowed growth *without* increased redistribution will result in real (as well as relative) deprivation among the "have nots," making class conflict more likely than ever before.¹¹ As Morrison (1976:299) notes, "Class antagonisms that are soothed by general economic growth tend to emerge as more genuine class conflicts when growth slows or ceases." Thus, in the long run the NEP suggests that Marx's predictions about class conflict may become more accurate, although for reasons Marx could not have foreseen.

In the short run, however, a very different possibility seems likely. The societal

pressures resulting from managed scarcity are such that large portions of *both* "haves" and "have nots" will push for a reversion to the economic (growth) synthesis. In fact, Morrison (1973) has predicted the emergence of a Dahrendorfian (i.e., non-Marxian) cleavage: "growthists vs. nongrowthists," with *all* those highly dependent upon industrial growth (workers and owners) coalescing to oppose environmentalists (who typically hold positions—in the professions, government, education, for example—less directly dependent on growth). The staunch labor union support for growth, and the successful efforts of industry to win the support of labor and the poor in battles against environmentalists, both suggest the emergence of this coalition. Somewhat ironically, therefore, support for continued economic growth has united capitalists and the "left" (used broadly to include most labor unions, advocates for the poor, and academic Marxists). Not only does this support reveal the extent to which most of the left has abandoned hopes for real *redistribution* in favor of getting a "fair share" of a growing pie, but it also reveals a misunderstanding of the distribution of costs and benefits of traditional economic growth.

The "Culture of Poverty" solidified: Sociologists guided by the NEP have not only questioned the supposed universal benefits of growth, but they consistently point to the generally neglected "costs" of growth—costs which tend to be very regressive (Anderson, 1976:30-31; Schnaiberg, 1975:19). Thus, it is increasingly recognized that the workplace and inner city often constitute serious health hazards, and that there is generally a strong inverse relationship between SES and exposure to environmental pollution (Schnaiberg, 1975:19). Further, in his study of the SES-air pollution relationship, Burch (1976:314) has gone so far as to suggest that, "Each of these pollutants when ingested at certain modest levels over continuing periods, is likely to be an important influence upon one's ability to persist in the struggle for improvement of social position . . . These exposures, like nutritional deficiencies, seem one mechanism by which class inequalities are

¹⁰ For example, wasteful consumption due to excess wealth and economic growth stemming from the investment of excess capital (for profit) would need to be halted, as would pressure for economic growth stemming from the unmet needs of the lower strata (see, e.g., Anderson, 1976:58-61; Daly, 1973:168-171).

¹¹ Managed scarcity and slowed growth are also likely to exacerbate tensions between developed and developing nations. This leads NEP-oriented sociologists (e.g., Anderson, 1976:258-269; Morrison, 1976) to see the future of international development quite differently than sociologists bound by the HEP (see, e.g., Horowitz, 1972).

reinforced." This leads him to suggest that efforts to eradicate poverty which do not take into account the debilitating impact of environmental insults are likely to fail.

Conclusion

We have attempted to illustrate the utility of the NEP by focusing on issues concerning stratification, for we believe this is one of many aspects of society that will be significantly affected by ecological constraints. As noted above, in the short run we expect tremendous pressure for reverting to the economic growth synthesis, for such a strategy seeks to alleviate societal tensions at the expense of the environment. Of course, the NEP implies that such a strategy cannot continue indefinitely (and the evidence seems to support this—see, e.g., Miller, 1972). Thus we are faced with the necessity of choosing between managed scarcity and an ecological synthesis.¹² The deleterious effects of the former are already becoming obvious; they help account for the trends described by Aronowitz and others. However, the achievement of a truly ecological synthesis will require achieving a steady-state society, a very difficult goal. As students of social organization, sociologists should play a vital role in delineating the characteristics of such a society, feasible procedures for attaining it, and their probable social costs. (See Anderson, 1976 for a preliminary effort.) Until sociology extricates itself from the Human Exceptionalism Paradigm, however, such a task will be impossible.

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¹² A point implicit in our discussion is worth making explicit: the NEP suggests that resource scarcities are unavoidable, but—as Schnaiberg's work indicates—societies may react to them in a variety of ways. Thus, as Schnaiberg (1975:17) suggests, sociologists should begin to examine the social impacts (especially distributional impacts) of *alternative* responses to scarcity.

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CONTEMPORARY CURRENTS IN MARXIST THEORY*

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This short paper presents a few of the issues which divide contemporary Marxists and shows how their debates relate to Marx's original work. In the first part, I discuss the family, law and the world system in the light of two notions of social structure. In the second part, I consider the dynamics of capitalism and the struggles between classes, races and genders as potential motors of change. This is followed by an outline of Marx's understanding of the persistence of capitalism; the notion of reproduction of social relations; and how the state becomes involved in the organization of struggles and in the preservation of the conditions of accumulation. In the final part, the past is examined for the light it may shed on the future. Has history a prior purpose, or telos? How did Marx and how do Marxists conceive of the transition from one period of history to another? What can we say about the transition to socialism based on the experience of the last hundred years?

Four convictions inform this essay. First, there is no Marxist alternative. There are only a plethora of Marxist alternatives—the accumulation of over a century of debate, struggle and revolution. Second, Marxism has overtaken Marx. But it still remains true to his methods, his categories and his concerns, and that is what makes it Marxist. Third, Marxism provides total portraits of the world, leaving no arena of social life unexplored. Fourth, Marxism systematically links the practical and the theoretical, the concrete and the abstract. Each particular theoretical perspective within Marxism is reshaped through the exploration of the problems it identifies. In this short paper I can only begin to sketch the basis for these assertions. For reasons of space I shall confine myself to contemporary issues within Marxism, and even then I shall cover only a small number of the current controversies. The paper is divided into four sections. Each section begins with a theoretical introduction, which is followed by one or two illustrations of the concrete research generated by different perspectives. In the first section, I deal

with two notions of social structure which produce different Marxisms, and I illustrate these by reference to two very different phenomena—the family and the world system. In the second section, after outlining the implications of the different notions of totality for the understanding of history, I examine class, race and sex as possible agents of change. In the third section, I discuss the idea of “reproduction” and examine it with reference to different theories of the capitalist state. In the final section, I discuss Marx's projection of history into the future and contemporary Marxist notions of passages out of the present.

TOTALITY: STRUCTURED OR EXPRESSIVE?

Whatever else they may be, Marxists are not empiricists.¹ This, of course, does not mean that they do not confront the empirical world. Rather, it means they do

* The ideas and themes of this paper have been influenced by Margaret Cerullo and Adam Przeworski. That they both fit inside the Marxist tradition says a great deal about the diversity of current Marxisms. I should like to thank three anonymous referees for their comments, and the staff of *The American Sociologist* for their patience, encouragement and criticisms of earlier versions of this paper.

¹ Lest there be any confusion, let me say at once this does not mean that Marxists are not positivists. Some are positivists, some are not. Inasmuch as they proceed deductively from certain premises and arrive at certain conclusions which they then attempt to relate to the concrete world, they are positivists. Such theories find their validity in *accounting* for what exists and what does not exist, what has been and what has not been, what will be and what will not be. Inasmuch as critical or any other theory justifies itself by reference to a preordained *goal* or *purpose*, it is not positivist. In contrast to both these approaches, empiricism draws conclusions about the concrete world on the basis of *induction*. It treats the world of appearances as the only world.

not measure for the sake of measuring. They do not mistake appearance for essence, ideology for reality. On the contrary, Marxists make a radical distinction between the two. They try to penetrate everyday experience to its underlying structure. By searching for a totality, they try to show that what appears as common sense—as natural, inevitable, and necessary—in fact rests on the existence of certain conditions which are not immutable but socially produced. The concrete empirical world is not “received” as given or viewed as separate from the apparatus used to understand it. Just as astronomers are not deceived by the apparent movements of celestial objects but seek to transform and explain appearances; just as Freud developed a theory which both transformed and explained the phantasmagoria of dream life; so Marxists following Marx attempt to unveil the hidden secrets behind lived experience, behind common sense, that is, behind the world of ideology. More specifically, they try to advance a *theory* of social structure; they try to show how networks of social relations into which we enter as individuals are produced by an *underlying structure*. This underlying structure then becomes the object of analysis: its dynamics, its contradictions, and its effects on the experience of particular individuals.

Within Marxism there are two distinct notions of how a social structure should be theoretically constructed. On the one hand, there is the idea of a Hegelian totality in which a single “essence” or dominant principle comes to pervade the entire society. Each part of the social structure becomes an expression of the whole, of the defining “spirit.” For Lukács (1971) “commodification” or “reification,” for Marcuse (1964) “one dimensionality,” for Braverman (1974) Taylorism (the separation of mental and manual labor and workers’ loss of control over their labor)—these are the dominant principles which both order and are expressed through social relations, not merely within the economic realm, but in leisure, in the family, in politics, in the cultural realm—in short, throughout capitalist society. On the other hand, there is the idea of a struc-

tured totality, in which a single part (the economic) determines the *relations* among all parts.² The economy, by virtue of its functional requirements (or, as Marxists say, conditions of reproduction), defines the contributions of different parts of society and thus the relations among those parts. Thus, it is a condition of existence of the capitalist economy that the legal system protect private property, that the family reproduce the labor force, that ideology legitimate capitalist relations, that the state enforce law and order, and so forth. The relations among the parts are established on the basis of their distinctive contributions to the working of the whole. Furthermore, the “function” of each part defines its form or structure, and in so doing, endows it with an autonomy and a logic of its own. Three illustrations of the different types of totality follow.

The Law

The “function” of the legal system is to define a set of formal rules which regulates and preserves capitalist relations. But in order to do this its operation must appear legitimate. The law must define and enforce “fair” rules. That is, it must treat all people as though they were equal and not distinguish between capitalists and workers; it must treat all property as though it were the same and not distinguish between property involved in the production of profit, such as machines, and property that is simply consumed unproductively, such as shirts. Moreover, changes in the law must appear to emerge from its own logic and not in response to particular interest groups. In short, the law possesses a coherence and autonomy of its own so it can effectively perform its function. It obeys principles and creates categories of

² The notion of a structured totality is most fully elaborated by the French Marxists associated with Althusser. The concept is developed, although not called by this name, in Althusser (1969) and Althusser and Balibar (1970). These writers have also coined the term “expressive totality” for their portrait of the “historicist school” of Marxism, in which they include Gramsci, Lukács and Korsch. They tend to caricature these writers in their attempt to elucidate their own “complexly determined” or “overdetermined” notions of totality.

people different from those in other parts of the totality.

By contrast, in an expressive totality the legal system is regarded as an expression of the single logic or essence that defines capitalism. Thus, if commodification and the universalization of exchange are regarded as the defining essence of capitalism, then the law will appear as an expression of that essence—it will operate on universalistic and impersonal criteria. There is little sense here of the legal system performing particular needs in the maintenance of capitalism. The legal system is not endowed with an autonomy of its own. Rather, its existence embodies the essence of capitalism.³ Functional interdependence is replaced by a principle of domination. I shall illustrate these differences with two further examples—the family and the world system.

The Family

From the point of view of the capitalist economy the family performs a number of definite and necessary functions. It maintains and renews the laboring population, that is, it reproduces labor power. It sends that labor power off to factories and offices. It prepares youth for the alienating experience of work. It socializes children for their future procreative and reproductive roles. It is a labor reservoir prepared for increased demands for industrial labor, as when women enter the wage labor force. It engages in consumption work, such as shopping. In the structured

totality, the family not only changes with the changing requirements of the capitalist economy, but it also possesses a structure of its own and therefore a certain relative autonomy which allows it to engage in the activities mentioned above (see, for example, Mitchell, 1971; Dalla Costa and James, 1972; Weinbaum and Bridges, 1976). Alternatively, the expressive totality may depict the family as a victim of commodification, in which the cash nexus enters the family; of reification, in which members of the family treat one another as objects; of Taylorization, in which domestic work becomes fragmented and deskilled; of whatever is defined as the essence of capitalism. That is, the essence of capitalism, its defining spirit, thrusts itself out from the core to penetrate the entire fabric of social life. Even the family, as one of the last arenas of potential resistance, succumbs, is stripped, and loses what little autonomy it had (for examples, see the Frankfurt Institute of Social Research, 1972; Braverman, 1974; Ewen, 1977).

The World System

If capitalism has managed to pervade and incorporate the farthest corners of social life within many Western societies, has it also incorporated the farthest corners of the world? Is the "world system" itself an expressive totality, in which each nation is subordinated to and devastated by the expansion of capitalism (Wallerstein, 1974)? Or is the world system a structured totality, in which different nations exhibit a political independence and an economy constituted out of a combination of capitalist and noncapitalist modes of production (Genovese, 1971; Mandel, 1975)? The answer depends in part upon what is being explained (the origin of capitalism, the dynamics of contemporary imperialism, national liberation movements, etc.) and in part upon the notion of capitalism. (Of course, the two are not unrelated.) Does capitalism refer to the particular relations men and women enter into as they transform nature, that is, a mode of production? Or does capitalism refer to the particular relations men and women enter into as they exchange the

³ Some works present an uneasy coexistence of both totalities. Thus, Baran and Sweezy (1966) emphasize the functional interdependence of the economy and the state in the first part of their book, while in the last chapters they shift into an analysis which portrays capitalist society as permeated by commodification and irrationality. They switch from monopoly capitalism conceived of as a mode of *production* to monopoly capitalism conceived of as a mode of *domination*. Similar tensions between expressive and structured totality can be found in Max Weber (Rheinstein, 1954). The modern legal system is seen both in terms of its autonomy and contribution to the needs of an industrial economy, and as an expression of the legal-rational spirit which defines the essence of the industrial society. The same ambiguity is found in the work of Talcott Parsons, who tries to stress both dominant values and functional subsystems.

products of their labor, that is, a mode of exchange?⁴

The latter position leads to a discussion of the progressive subordination of peripheral regions of the world to the core regions (Baran, 1957; Frank, 1969; Wallerstein, 1974), and to the examination of the forms of unequal exchange between powerful capitalist nations and the third world (Emmanuel, 1972). What is missing from such analyses is a specific elaboration of the response and possible resistance to the spread of capitalism, or at least to the uneven development capitalism brings in its wake. The underdeveloped world becomes a dependent appendage of the advanced world with little power to resist subordination. Nairn's (1977) analysis of nationalism is one of the few attempts to come to terms with modes of resistance. Nationalism—always a thorn in Marxist flesh—can be understood, argues Nairn, as the attempt of a rising or weak bourgeoisie to simultaneously harness resources for the development of capitalism and resist subordination to a powerful international bourgeoisie. To this end, the emergent capitalist class of the periphery and semiperiphery mobilizes the only resource at its disposal, namely, the people. It does this through the ideologies of nationalism and populism. Nairn convincingly demonstrates the link between the combined and uneven nature of capitalist development, and the appearance of Scottish nationalism and the nationalism of Western Europe in the nineteenth century. Without much effort his ideas can be extended to contemporary Asia, Africa and Latin America. Yet the particular form and content of those nationalisms have still to be examined.

This can be more appropriately accomplished through a vision of the "world system" as composed of a combination of capitalist and noncapitalist modes of pro-

duction (see, for example, Lenin, 1960). The particular arrangement of modes of production (capitalist, petty commodity, primitive communist, etc.) within peripheral or semiperipheral territories provides a specific material basis for different forms of resistance, particular types of nationalist movements, and so forth. What becomes significant in this structured totality is not the erosion of all precapitalist modes of production by capitalist modes of production, but the political and ideological forms which facilitate the coexistence of precapitalist and capitalist modes of production (Wolpe, 1972; Laclau, 1971) and the transfers of labor and surplus from one mode of production to another through, for example, a system of migrant labor. The second perspective, therefore, emphasizes the *interdependence* of precapitalist and capitalist modes of production whereas the first perspective stresses the *destruction* of precapitalist modes of production.

HISTORY: WITH OR WITHOUT A SUBJECT?

Like social structure, history also has to be constructed. It is not received as a succession of events but rather is constituted out of its premises. And the first premise is that men and women must be able to live in order to make history, that is, they must transform nature into the means of their existence. Therefore, history is conceived as the succession of ways of producing the means of existence, that is, as the succession of modes of production—primitive communist, ancient, feudal, capitalist, etc. Each mode of production is defined by a set of relations into which men and women enter and the corresponding form of consciousness. Accordingly, history has two aspects: (1) the dynamics of a given mode of production—how it changes while remaining the same; and (2) transitions from one mode of production to another. I shall consider the latter in the final section, while this and the following section will be largely devoted to the dynamics of capitalism.

Marxists constitute the history of the capitalist totality out of its essence or underlying structures. Thus, the expressive totality sees the history of capitalism as

⁴ A similar issue has gained prominence in the debate over feudalism and the transition from feudalism to capitalism. Is the distinction between feudalism and capitalism to be seen in terms of production for use rather than production for exchange or in terms of the extraction of surplus through rent rather than through wage labor? See the classic contributions in Hilton (1976).

the unilinear unfolding of an essence, a single principle (commodification, Taylorism, etc.), as it invades and incorporates ever greater expanses of social life. As portrayed in the works of Marcuse (1964), Lukács (1971), Aronowitz (1973) and Braverman (1974), capitalist domination possesses an ineluctable logic which eliminates resistance, absorbs alternatives and assimilates critique. Because it leaves largely unexamined the *problematic conditions* of domination, this perspective inevitably leads towards utopianism, determinism and pessimism. Therefore, commentators such as Marcuse tend to embrace almost any potentially emancipatory challenge to domination the occasion offers—students, new working class, women, etc.

The structured totality produces a very different notion of social change. Here history is marked by an indeterminacy. It is not unilinear or unidimensional but uneven; it is the combination of the disparate histories of its separate parts, namely the political, the ideological, the economic, and so on. Since these parts move with their own relatively autonomous dynamics, revolutionary situations or conjunctures can appear with a degree of unpredictability. Thus, the expressive totality directs our attention to *arenas of resistance*, that is, to particular places; the structured totality focuses on *times of crisis*, that is, particular conjunctures.⁵

Thus far we have conceived of the history of capitalism as the unfolding of some irrevocable logic or combination of logics. But logics, structures, and principles do not *make* history. Who does? And furthermore, does it matter who does? Does history take place behind our backs, beyond our control, or are there agents who consciously shape the movement of history? This is the terrain of classical Marxist debate expressed through the dichotomies of freedom and necessity, revolution and science, voluntarism and determinism. Is the historical process an

unwinding of irrevocable laws inscribed in the structure of the mode of production, such as the falling rate of profit, whose pace may be temporarily halted or even reversed, but whose ultimate direction and destiny is preordained? Or are there no such laws and is history contingent on unconstrained class struggle? Of course, these polarizations are crude, presenting false dichotomies which Marx warned against—men and women make history, but under conditions not of their own choosing. Marx acknowledged these constraints, but he still regarded class as the agent and class struggle as the motor of history. What then has become of Marx and Engels' opening to the *Communist Manifesto*: "The history of all hitherto existing society is the history of class struggles"? Let us see.

Class: Historical Actor or Sociological Category?

Within the Marxist tradition two notions of class have emerged: class in itself—a sociological *category* designating particular places in relation to the means of production; and class for itself—a social *force* which makes history, perhaps even marches through history. The problem is to connect the two concepts both in theory and in practice. Marx tended to presume some inevitability about the association of particular places in the social structure with particular historical actors. And there have been some brilliant "demonstrations" of the logical and historical necessity for a class in itself to turn into a class for itself. Both Lukács (1971) and Thompson (1963)—one in an abstract theory and the other in a concrete account—identify the proletariat as the subject of history, the subject whose presence comes to dominate all areas of society. The totality comes to be identified with the emergence of the proletariat, in whom science and revolution, necessity and freedom, object and subject, are unified. But history has confounded brilliance and created, if anything, a widening gap between the two notions of class. After all, what happened to the English working class after 1830, where Thompson's account stops? What

⁵ I follow Przeworski (1977:39) in viewing crises as "... moments of uncertainty, the periods of decision when forms of organization of society become the object of struggles and when relations of organized physical force come to the fore."

happened to its revolutionary consciousness? What has happened to revolutionary proletariats of Western Europe during the last fifty years? Faced with a proletariat which is not revolutionary, or with agents of social change who do not constitute a class, that is, who are not defined by a unique relationship to the means of production, Marxists find themselves embracing one notion of class or the other—hanging from one pole while stretching for the other as it recedes into the distance. Those who vacillated have dropped into the gorge between the two.

Accordingly, some Marxists are beginning to develop different class maps of the capitalist social structure. Erik Wright (1976), for example, treats the United States as a combination of capitalist and petty commodity modes of production, which gives rise to three classes: capitalists, workers, and petty bourgeoisie. By introducing the notion of contradictory class location to represent "intermediary" positions between these classes (small employers, managers, supervisors, etc.), he has begun to forge new tools for illuminating the capitalist social formation.⁶ These new Marxist class categories seem to promise a new theory of social structure, that is, of the production, linkages and dynamics of places in that structure which emerge from the tendencies of the capitalist mode of production and its reproduction requirements. For example, how do changes in the labor process (proletarianization and expropriation of skills) and in the economic structure (rise of service industries) create and destroy positions in the social structure (Braverman, 1974)? However, it should not be forgotten that such formulations lead right back to history without a sub-

ject. They ignore the fact that the production of places in the social structure becomes the object of struggle. Struggles among classes and other groups must be incorporated into a theory of social structure.

Alternatively, some Marxists cling to class as a historical force (Poulantzas, 1973; Przeworski, 1976; Balibar, 1977). Class in itself drops out, leaving only class for itself. Unfortunately, losing sight of the location of actors in relation to the means of production may lead to lumping together workers and capitalists into a single "class," or constituting women and blacks as a class denuded of its explicit link to the economic. To avoid such a predicament it may be necessary to bring back a weak notion of class in itself. Thus, one possibility is to restrict class as a historical force to politically organized agents of production. But these still may not be the significant historical actors. A second possibility is to regard historical actors as coalitions or alliances of classes defined in terms of agents of production. This would lead to a discussion of the organization and reorganization of relations among classes. The utility of these approaches would have to be explored empirically.

Sex and Race: The Achilles' Heel?

Not surprisingly, some Marxist thinkers (particularly in the United States) have been content to abandon class altogether, although this may have cast them outside the ambit of Marxism. Others, in trying to understand the quiescence of the working class, or at least the absence of revolutionary consciousness, have turned to gender and race as alternative sources of polarization and struggle, and as historical forces in their own right. Marx expected relations between classes to assimilate relations between nations, sexes and races, but for contemporary Marxists this is no longer a viable position. The creation and reproduction of these relations represent a theoretical challenge they have met with varying success. They have posed a number of questions. In what way, if any, can relationships of gender and race be linked to class and modes of production? Or do the social distinctions of gender and

⁶ It should be noted that his notion of contradictory class location does in fact smuggle back, however surreptitiously, class as a historical force, and the problem of "class in itself/class for itself." For Wright, the significance of the contradictory class location rests on the ambiguity of the incumbent's relationship to two "fundamental" classes, that is, ambiguity as to which class the incumbents will support in class struggle. This indeterminacy is resolved by the intervention of political and ideological factors. Therefore, Wright's class map is by no means a purely "sociological" map, but contains all sorts of assumptions about historical actors.

race transcend any periodization of history by class and mode of production? Are relations between men and women prior, historically or logically, to the relationship between dominant and subordinate classes?

Of course, this is no abstract issue. With the disappearance of classes under a putative communism, can we also be assured of the disappearance of other forms of oppression based on gender and race? What is the link between class and gender or class and race, not at an empirical level but at a theoretical level? In response to this problem, one avenue of investigation has been to ascertain whether there is male dominance in all precapitalist classless societies: whether there have been societies in which men did not dominate women (see Reiter, 1975 and Rosaldo and Lamphere, 1974, for two different sides of the debate over the universality of male dominance). The debate appears inconclusive due to ambiguity in the concept of dominance. Moreover, although such studies may shed light on the relationship between gender and oppression under primitive communism, it is quite another matter to generalize their results to any future form of communism.

Similar arguments can be made with respect to race, although they have been less well elaborated because of the seemingly accidental appearance of racial divisions. Jordan (1968) has traced racism to psycho-social attributes of the precapitalist era and before the emergence of slavery in the United States. But these issues do not broach the question critical to a Marxist understanding of race, namely, is it more helpful to look at the continuity of racial oppression through history and across modes of production, or is it more appropriate to examine racial oppression in terms of the particular mode of production in which it is found, such as slavery (Genovese, 1976) or capitalism? The question is not whether capitalism is the original source of racism, but whether the form racism assumes under capitalism is sufficiently different from its form under slavery to warrant an entirely separate treatment.

How have Marxists linked racial divisions, oppression and discrimination to

the capitalist mode of production? Theorists of the dual economy, such as Harrison (1972), have tentatively suggested that racism may be reproduced through a segmented labor market linked to different fractions of capital (monopoly, competitive and service sectors). There is a tendency, it is argued, for blacks to fill places in the competitive and service sectors, while whites are awarded preference in the monopoly sectors. That the matching of race and labor market is not perfect only serves to obscure the class basis of race relations. Alternatively, race could be viewed in terms of modes of reproduction of labor power. The ghetto represents a particular system of reproducing labor power, just as the Mexican village constitutes a different mode of reproducing labor power. In both instances, ethnic or racial labeling obscures the different ways through which the labor force is maintained and renewed. Yet another possibility is to look at patterns of race relations as the product of the interrelationship among different modes of production. Thus, Wolpe (1972) argues that the apparatus of South African apartheid is a mechanism of reproducing a precapitalist mode of production alongside a capitalist mode of production. Marxism has not, and, I would argue, cannot develop a general theory of race relations. Instead, particular or local theories are generated to explain how different forms of race relations express and conceal the specific conjuncture and context in which they are produced and reproduced.

Therefore, the discovery that racism and male dominance are universal attributes, or at least exhibit a continuity across modes of production, would not of itself deal a death blow to Marxist analysis. But it would mean that two types of analyses would have to be developed: one concerned with understanding the reasons for the generality of the phenomenon irrespective of the historical context, and a second concerned with the particular forms it assumes in relation to any given mode or modes of production.⁷

⁷ Thus Rubin (1975), recognizing the specific subordinate roles that women occupy under capitalism, seeks to explain why it is women rather than men

REPRODUCTION AND DOMINATION

Whatever its dynamics, capitalism both changes and remains the same. How did Marx and how do Marxists talk about the continuity over time of those social relations which define the capitalist mode of production? Unlike much contemporary sociology, Marx did not "solve" the problem of order by focusing on a commitment to capitalism generated by the internalization of certain values and norms.⁸ Values and norms are the product of social relations.⁹ Capitalists accumulate and workers work because they are enmeshed in a set of social relations indispensable and independent of their will. But once established, social relations do not spontaneously maintain themselves; they do not persist of their accord, but rather have to be continually perpetuated, that is, reproduced. This notion of reproduction of social relations can be illustrated with a simple example. Within the capitalist mode of production there are two fundamentally different places. *Workers*, dispossessed of the means of gaining an independent livelihood, have to sell their capacity to work—their labor power—to *capitalists*, who own and control the means of production. In selling their labor power for a wage, workers renounce their power to appropriate the products of their

who fill those places. To answer this question it is necessary to go beyond capitalism and to seek the source of allocation of women to subordinate positions in their exchange through marriage rules. This, she argues, transcends all modes of production, while the concrete forms of male dominance are linked to a particular mode of production.

⁸ This is a juncture where Marxism has frequently parted with Marx. Thus, a major contribution of the Frankfurt School has been the Marxist appropriation of Freudian psychology. It is interesting to note the convergence of the psychoanalytic descriptions of contemporary society as found in Marcuse (1955) and in Parsons (1954). Of course, they differ in their evaluations of the potentiality for transcending the repressive aspects of capitalist society.

⁹ This is not to say that values and norms, or, as Marxists would say, ideology, do not have a structure and autonomy of their own. They do. Moreover, ideology is not uniquely determined by social relations. Indeed, ideological struggle reflects the ambiguous relationship of ideology and social relations. Nor does this mean that ideology is unimportant. On the contrary, as Marx wrote and Gramsci continually emphasized, it is ideology that shapes class struggle.

labor. Instead, the capitalist expropriates the products of labor as commodities and transforms them into (gross) profit and future wages. In other words, as a result of producing a "thing," workers produce (earn) a wage which allows them and their families to survive, but only until the next working day; and they produce a profit which not only keeps the capitalist rich but also keeps him in business, and therefore guarantees the prospect of future wages. The production of things, therefore, produces on the one side the worker and on the other side the capitalist, that is, it *reproduces* the relationship between capital and labor.

This, in fact, is Marx's conception of the reproduction of capitalist relations. Marxists have proposed a number of reasons why it is no longer adequate to look upon the relations between capital and labor as automatically reproducing themselves through the production of commodities. I shall deal here with only two. First, there are relations among capitalists which tend to make the production, circulation and consumption of commodities more and more difficult. Second, there is a tendency for the reproduction of relations between capital and labor to generate different forms of class struggle, which in turn threaten to undermine those relations. These two sets of tendencies towards the dissolution of relations of production give rise to two types of theories of the state: interventionist theories, which attempt to explain how, why, and when the state is able to counteract tendencies toward economic crises; and theories which aim to show how class struggles are organized, contained, or suppressed by the state.¹⁰

Interventionist Theories of the State

What are the "crisis" tendencies of the capitalist economy which threaten to

¹⁰ Tendencies toward economic crises and class struggles have been counteracted not only by the state but also by changes in relations among capitalists and in the labor process, both brought about by the emergence of the large corporation. See, for example, Baran and Sweezy (1966) and Braverman (1974). I have commented at length on these changes elsewhere (Burawoy, forthcoming [a] and [b]).

undermine the reproduction of relations of production? The most conventional are the various elaborations of Marx's "tendency for the rate of profit to fall" as a law inscribed in the structure of the capitalist mode of production. However, no matter how sophisticated the mathematics in these elaborations (for example, Yaffe, 1973), it is always possible to discover some empirically untenable assumptions underlying the inferences. The question rests on the relative strength of the tendency for the rate of profit to fall and the counter-tendencies (such as increasing the rate of exploitation, capital saving innovations, cheapening raw materials, etc.), in which the state plays an important role. There does not seem to be any obvious way of demonstrating that the tendencies are stronger than the counter-tendencies, so many look upon the movement of the rate of profit as the product of contingent historical forces (Mandel 1975).

By contrast, Baran and Sweezy (1966) maintain that the falling rate of profit may apply to the era of competitive capitalism, but under monopoly capitalism it is replaced by the tendency for the absolute level of surplus to rise above the capacity of capitalism to absorb it. The problem is not too little surplus but too much surplus. Of course, the two "laws" are by no means incompatible, for the amount of surplus can increase relative to consumption while falling relative to the quantity (measured in socially necessary labor time) of labor and capital employed. Baran and Sweezy argue that the tendency towards over-production brings into play state mechanisms for surplus absorption, such as the expansion of military capacity. Hence, they point to underlying economic imperatives leading towards the warfare state.

Other crisis theories focus on the problematic nature of exchange and circulation, in particular the problem of ensuring that what is required for consumption (productive or unproductive) is also produced in the right proportions (Mandel, 1975). How is this accomplished through the market? When the market fails, as it does under monopoly capitalism, what agencies intervene to assure proportionality? O'Connor (1973) suggests that

the state must intervene to provide forms of "social capital" (roads, transportation, communications, research and development, subsidized housing, etc.) to guarantee those prerequisites of accumulation which individual capitalists cannot afford.

O'Connor argues that the state is also responsible for "legitimizing" capitalism through the distribution of concessions to the working classes (welfare, social security, etc.). But the costs of social capital and social expenses (concessions) tend to outstrip revenues, precipitating a "fiscal crisis of the state." Although economic in origin, the crisis manifests itself in the political arena. Yet it is not clear how the crisis can be recognized and whether there is an inherent tendency towards its exacerbation.

Habermas (1975) extends O'Connor's ideas to other realms, claiming that there are tendencies towards economic, rationality, legitimation and motivational crises, but he does not explain why, when, how and under what conditions these crises appear. Nevertheless, the idea that contradictions can be displaced or externalized from one sphere to another is a definite advance on earlier formulations of the Frankfurt School, which were based on the assumption of the durability of the capitalist economy, focused on the cultural realm, and chose to ignore the dynamics of the economy.

All these theories assume a similar form. A crisis is identified, a functional gap discovered, a contradiction revealed, and the state is invoked as the agency of restoration. This is an unsatisfactory functionalism. First, each theory of the contradictions of the capitalist economy gives rise to a different theory of the state, which means that Marxists have to direct attention to developing more comprehensive theories of the economy: nothing short of rewriting the three volumes of *Capital*! Second, the conditions under which the state endeavors, or even possesses the capacity, to counter-act crisis tendencies are left unformulated. Such questions revolve around the issue of class struggle, which has yet to be systematically incorporated into these frameworks.

Class Struggle Theories of the Capitalist State

The second set of theories, inasmuch as they examine the relationship between class struggles and the state, complement the interventionist theories. These theories have emerged out of different interpretations of Marx and Engels' celebrated formulation in the *Communist Manifesto*: "the executive of the modern state is but a committee for managing the common affairs of the whole bourgeoisie" (Tucker, 1972:337). The conventional understanding of this passage is that the state acts as a coercive instrument for the dominant class (Miliband, 1969). The state is defined in terms of its various branches or "apparatuses"—the military, the police, the judiciary, the government, civil service, and so on. This *instrumentalist* perspective is linked, albeit weakly, to the notion of the expressive totality, in which all arenas of society are subordinated to the power of capital, thereby losing their individual autonomy.

In contrast, theories linked to the structured totality examine the *functions* of the state, that is, they focus on the "common affairs of the whole bourgeoisie" rather than on the *institutions* through which those functions are carried out. Poulantzas (1973), for example, translates the common affairs of the whole bourgeoisie into the unity and cohesion of the entire social formation. To preserve this unity and cohesion, he argues, it is necessary for the state to assume a *relative autonomy* with respect to individual capitalists or fractions of capital (finance, competitive, monopoly, commercial, etc.). For, in siding with this or that capitalist or group of capitalists, the state may jeopardize the *common* interest of all capitalists, that is the interests of the *capitalist class*, the interest in the reproduction of capitalist relations of production, and in organizing class struggles in ways which do not threaten the capitalist order. This is not to say that the state never sides with a particular fraction of capital. It is often forced to do so to protect the common interest of the capitalist class, for example, in subsidies to agriculture. On the other hand, when the state

becomes an instrument of one or another fraction, and this obviously does happen, then the ability of the state to preserve its legitimacy is impaired.

The state must also assume an autonomy vis-a-vis the entire capitalist class. For the state must be in a position to grant material concessions to subordinate classes at the expense of the immediate economic interests of the dominant classes, for example, in the New Deal; to erect a hegemonic ideology which presents the interests of the dominant classes as the interests of all; to constitute the citizen/individual as the essential social category which the state establishes and recognizes in its structure so as to disorganize the dominated classes; and so on. Even individual branches of the state must operate with their own autonomy if they are to secure the consent of the people to the capitalist system, as in the Watergate hearings and the operation of the Watergate Special Prosecution Office. Poulantzas (1974, 1976) extends these formulations to examine the concrete forms the capitalist state can assume under, for example, fascism, dictatorships and parliamentary democracy. In each instance, he tries to identify a characteristic relative autonomy of the state as determined by different arrangements among the dominant classes and the balance of power between dominant and subordinate classes. Then the conditions can be examined under which a relative autonomy breaks down and yields to a state that becomes the instrument of a particular fraction of the dominant economic class. Chile, before, during and after Allende, provides an interesting case study of the ways in which different apparatuses of the state can be "seized" by different classes and of the implications this absence of relative autonomy has for the survival of a particular type of regime.

The weakness of the structuralist view of the state, as it is presently formulated, is its functionalism. How is it that the state does what it is supposed to do? How does it secure and protect its relative autonomy? How does it dispense concessions? What are the mechanisms through which it preserves the hegemony of the dominant classes? Moreover, it is here

that the instrumentalist perspective appears to be strong because it provides an explanation for the policies executed by the various branches of the state. Unfortunately, many of its assumptions are too crude. For example, Miliband (1969) incorrectly infers the existence of a cohesive, class conscious, enlightened bourgeoisie based on a relative homogeneity of attitudes, education, social origins, and so on. Irrespective of their common backgrounds, capitalists, both as groups or fractions, and as individuals, compete and conflict with one another, and thereby continually jeopardize their common interests. Furthermore, the state frequently acts in opposition to the declared and defended interests of the capitalist class or its fractions. The struggles over the Factory Acts, or the day to day commentary in the *Wall Street Journal* are obvious illustrations of the state acting with an autonomy of its own. In an attempt to rescue an instrumentalist perspective, some have followed the theorists of the corporate liberal state, such as Kolko (1963), Weinstein (1968), and Williams (1961) who postulate and try to demonstrate the existence of a hegemonic and enlightened fraction of the capitalist class which directs the state for the preservation of the interests of the whole capitalist class, even where this involves economic sacrifices. A second problem confronting the functionalist formulation is the mapping between function and concrete institution. Are all institutions which promote the functions of the state also part of the state? The family, for example, clearly contributes to the cohesion of the entire social formation, but does that necessarily place it within the orbit of a state apparatus?

Significantly, the two theories—structuralist and instrumentalist—offer very different perspectives on the “transition to socialism” and the current debate over “Euro-communism.” In writing about a *state in capitalist society* Miliband implies that the state he describes can be wielded by any economically dominant class (bourgeoisie or proletariat) to protect its specific interests. If the proletariat or its representatives can only seize the state, by electoral or any other means,

then socialism can be inaugurated. Poulantzas (1973) and Balibar (1977), by contrast, refer to a *capitalist state* as distinguished from a feudal state or a socialist state. Because of its very structure, because of the social relations of which it is composed and which are independent of the will of those who (wo)man its apparatuses, the capitalist state will continue to protect and reproduce capitalist relations of production even if a socialist or communist party gains power. Thus, conquering or gaining access to the state through electoral means cannot lead to socialism since the working class party, when it takes over the government, becomes a prisoner of the very system it attempts to overthrow. Rather, in the “transition to communism,” the capitalist state has to be dismantled and replaced by a socialist state which has the capacity to dissolve itself.

THE FUTURE AS HISTORY

For sociology, history ends with capitalism.¹¹ For Marxism, history ends with communism. Peculiar to all Marxisms is a vision of the future which is fundamentally at odds with the present. But how is that future to be realized? Marx uncovered a logic or telos to history, to the succession of different modes of production, which made socialism and communism the inevitable successors of capitalism. His logic rested on the development of the forces of production, that is, the increasing capacity of human beings to transform nature. This notion of progress is what linked past, present and future. Marx also sketched the general process by which one mode of production both *made necessary* and *laid the basis* for the next mode of production. As the forces of production—the manner of transforming or appropriating nature—advance, so they enter into conflict with the relations of production—the way surplus is appropriated by a dominant class, or, as Marx wrote, the particular

¹¹ To refer to the “post industrial society,” “post capitalist society,” “advanced industrial society,” etc., as “socialism” is to denude that concept of its Marxist meaning.

way of pumping surplus out of the direct producers. When the relations of production are no longer compatible with the development of the forces of production they become so many fetters and are burst asunder. A period of social revolution is inaugurated and class struggle becomes the driving force in the transition to a "higher" mode of production. The new relations of production become forms of development of the productive forces until again an incompatibility arises, and another revolutionary period brings forth the next mode of production.

How does Marx apply this theory to the capitalist mode of production? Individual capitalists privately appropriate unpaid labor, or what Marx calls surplus value, in the form of profits, realized through the sale of commodities in the market. Competition among capitalists drives them, on pain of extinction, to the continual transformation of technology and of the labor process, that is, of the productive forces. The transformation of labor increasingly assumes a "social" or collective form with the interdependence of labor increasing at the same time as its homogeneity. The process of accumulation leads on the one side to the concentration and centralization of capital, and thus to the elimination of small employers; and on the other side to the production of surplus laborers as the capital intensity of technology increases. A polarization grows between those who own the means of production and who privately appropriate surplus, and those who own only their labor power and who collectively appropriate nature. At the same time, the productive forces develop a power beyond the capacity of society to consume their products, causing crises of overproduction and hindering further expansion of those productive forces. Crises of overproduction combine with a decline in the rate of profit (linked to increasing proportion of capital relative to labor) to lay the objective basis for the inevitable dissolution of capitalism. In addition, the expansion of the productive forces creates the foundation for socialism because it presents the possibility of a regime of plenitude in which individual and collective talents can be developed to the full

through engagement in varied types of work. However, the realization of these potentials (that is, the overthrow of capitalism and the construction of socialism) rests not only on the development of objective contradictions, but also on the level of struggle which is shaped in ideological and political arenas. This was Marx's theory.

The entire corpus of twentieth century Marxism—from Kautsky to Colletti, from Lenin to Althusser, from Gramsci to Habermas, from Luxemburg and Trotsky to Mao, from Lukács and Korsch to Marcuse—can be understood as reformulating and reinterpreting this theory of the transition to socialism. Informed by the ability of capitalism to confound Marx's prognoses, Marxists have increasingly looked upon his optimism with a certain ambivalence. And just as Marx sought to justify his vision of the future in a teleology, a hidden (or not so hidden) purpose of history, so Marxists have returned to history as a means of reexamining passages out of the present. How, then, do Marxists conceive of transitions from one epoch to another? They have questioned the idea of one mode of production being born in another. Anderson (1974) suggests that the feudal mode of production arose out of the catastrophic collision and fusion of two dissolving modes of production, namely the primitive Germanic and the ancient Roman. In the transition from feudalism to capitalism, according to Balibar (1970), the meeting of capital and wage labor, that is, the genesis of capitalism, has to be conceived of as occurring *outside* the decline of the feudal mode of production. In dislocating the genesis of one of mode of production from the dissolution of its predecessor the idea of progress is lost. There is no theoretical reason why feudalism could not have been followed by the ancient mode of production, or even by socialism. Such a position is taken to its logical conclusion by Hindess and Hirst (1975). In their analysis of precapitalist modes of production they argue that while history may offer a sense of alternatives and thus of what is possible, at the same time there is no logical or teleological way of ordering those possibilities. History is

not a fortune teller. What is left is a radical indeterminacy in the transition from one mode of production to another. Outcomes, including socialism, depend on struggle.

Gramsci's (1971) formula—"pessimism of the intelligence and optimism of the will" (taken from Romain Rolland)—resonates not only with historical studies but also with contemporary analyses of capitalism. I have already referred to the gap between class as a sociological category and class as a historical force, and to the capacity of the state to cushion and counter-act crisis tendencies through the organization of politics and ideology. Other critics have invaded Marx's scheme at an even deeper level, arguing that the forces of production develop in ways that reinforce and reproduce rather than threaten capitalist relations. Gorz (1976), for example, shows how the labor process, and even technology, can serve to prevent the formation of class consciousness through the fragmentation, atomization and hierarchization of relations in the factory and office. These factors not only divide the working class into individuals and competing groups, but also obstruct the penetration of immediate experience to the totality of relations which shape people's lives. Gorz (1976), Marglin (1976), and Braverman (1974) are ambiguous in their assessment of capitalist technology—whether or not it has an emancipatory potential and could be used under socialism. Marcuse (1964), on the other hand, maintains that the very technology is tainted. Capitalist productive forces, far from being neutral or innocent, embody a form of domination incompatible with notions of a true socialism. Socialism requires socialist machines and even a socialist science. Capitalist technology is irretrievably contaminated. Responding to Marcuse in a now celebrated debate, Habermas (1970) tries to restore neutrality and continuity to the development of the productive forces. In themselves they are neither innocent nor guilty. Thus Habermas can redirect his attention to the political as the arena of emancipation. His vision of the future rests on a return to genuine consensus

politics—what he calls the repoliticization of the public realm.

All these sorties into world history, the dynamics of capitalism and alternative technologies have been prompted by very definite historical experiences of the twentieth century. From among these, attempted transitions from capitalism to socialism hang heavily in the minds of Marxists—in the Marxist collective consciousness. An important lesson of the last hundred years is that it is one thing to speak of alternative futures or even of repressed potentialities in the present; it is quite another matter to move towards such visions even when a revolutionary crisis presents itself. Strong socialist and communist movements in Germany and in Italy led not in the direction of socialism, but in the direction of fascism. Contemporary events also illustrate the precariousness of left wing movements fighting a capitalism constituted on a world scale. The examples of Chile and Portugal suggest the ease with which counter-revolution, restoration or dictatorship can be established.¹² It remains to be seen what will happen in Spain, France, Italy and Greece. Labor governments, such as the one in England, find themselves fighting for the survival of capitalism.

Even if counter-revolution in any of its guises and the social democracy of welfare capitalism are averted, the path to socialism is still filled with daunting and seemingly insuperable obstacles. Some form of capitalist restoration is always possible, even likely. Widespread disillusionment with the unfolding of events in the Soviet Union has made Marxists even more cautious in their speculations and prognoses. In such a historical context, critical theory affords an understandable retreat, particularly in the United States where the future appears so hopeless. By stressing the widening gap between what is and what could be, critical theory aims

¹² The possible resurgence of fascism as a reactionary response to the strengthening of European socialist and communist parties has prompted Marxists to reexamine the origins and nature of National Socialism. See, for example, Mason (1968); Rabinbach (1974); Poulantzas (1974); Abraham (1977); Goldfrank (1977).

to undermine the seeming naturalness and inevitability of everyday life and reveals common sense as ideology. But in bridging the divide between "reality" and potentiality, between the present and the future, critical theory has little to offer.

In nations such as France and Italy, with traditions of revolution and class struggle, Marxist debate takes place on a different terrain than in the United States, and directly confronts the issues of the transition to socialism. Accordingly, new directions in Marxist studies revolve around the reexamination and reinterpretation of the history of the Soviet Union. It is no longer enough to "critique," condemn, or lament the fate of the October Revolution; or to lay the blame at the feet of individuals or accidents of history. However disturbing it is, Marxists have been forced to examine precisely how, when and why it went wrong (see, for example, Bettelheim, 1976). But these reconstructions have to be a real history—a Marxist history—not a crude vindication of the status quo or an apology for the Soviet ruling class. Such endeavors, combined with the recent withdrawal of many European communist parties from beneath Soviet hegemony, can only augur well for the extension and deepening of Marxist discourse on the prospects and nature of socialism. Presumably, that has something to do with its realization.

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SOCIOLOGY AND GENERAL SYSTEMS THEORY

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General systems theory (GST) is a strategy of inquiry. It integrates such diverse areas of research and theory as phenomenology and symbolic interactionism with functionalism, conflict theory and various new perspectives. It is useful in applied research and theory construction. GST avoids the problems of reification, reductionism, metaphysical dualism, linear thought and homeostasis which are endemic to contemporary sociology, particularly to functionalism. It pursues a processual logic of relationships rather than a formal Aristotelian logic. Its radical empiricism escapes barren abstractions and incorporates the concept of positive feedback as a complement to the idea of negative feedback implicit in functionalism. If its potential problems are anticipated and avoided, and if it is used properly, general systems theory offers many advantages as a research strategy.

Sociology has been intensely concerned with the issue of "systems," and a great deal has been written on the subject. There are literally dozens of different systems perspectives, each developed by particular exponents of a given point of view. Some theories stress the patterns of social organization, others the dominant cultural configurations, still others emphasize factors such as religion, economic circumstances or geography. Into this tangle has come general systems theory (GST), a framework which has developed across a variety of sciences but which hopes to deal with a major problem common to them all: the scientific treatment of organized complexity. Just as there are many different "systems theories," there are some wide variations within GST itself. No one proponent can hope to speak definitively. My intention in this brief space is to outline something of the ongoing enterprise of GST. If what follows is at all successful in stimulating interest within our discipline, then I may be forgiven for a treatment which will undoubtedly overlook some of the most important features of GST while perhaps overemphasizing others.

General systems theory is "essentially a cluster of strategies of inquiry" (Berrien, 1968:13) which attempts to overcome the limitations of traditional positivism without sacrificing conceptual clarity, parsimony, precision and pragmatic attention to the natural world (Laszlo, 1972). It is not a new theory, but a paradigm shift, and it can be applied to the

sciences generally. Although GST is directly applicable to problems of systems technology (Von Bertalanffy, 1968), it must not be confused with the conventional practice of "Systems Analysis," which has retained the reductionistic narrowness so characteristic of extreme positivism (Boguslaw, 1965). Those attracted to GST find it a particularly powerful framework for applied research and theory construction. It can contribute to traditional functionalism and conflict theory, as well as to such schools of thought as symbolic interactionism, and may eventually help sociology to deal more effectively with the problems of these different traditions. I will outline some potential contributions of GST, taking examples from two important areas of research—social deviance and social change. I will argue that GST can begin to resolve some of the major dilemmas which have impeded research and theory construction in sociology. Among these are the tendency toward reification, the constant threat of reductionism, the persistence of metaphysical dualism, the dominance of narrowly linear thought patterns, and the limitations inherent in the concept of homeostasis.

Beyond Reification

Western epistemology has been plagued by what Whitehead (1929) has called the "bifurcation of nature"—that arbitrary division of the realms of mind and matter which produces either idealists or

materialists. Both of these schools of thought are based on an essentially Aristotelian logic. This is a logic preoccupied with taxonomy, which proceeds by isolating analytical categories and ordering these abstract categories by a method of subsumption and aggregation similar to the way bricks are cut out and then laid upon one another to form an artificial wall. System builders who work within this tradition try to divide reality into internally consistent, boundary-maintaining categories, and then to construct relatively timeless, closed systems out of these parts. Idealists may engage in rhetorical construction, often ending in scholasticism. Materialists may attain to the taxonomic achievements of, for example, a Linnaeus. But in both cases, further progress is blocked. The source of the impasse is the latent tendency toward reification of categories.

As van den Berghe (1963:98) has stressed, the most serious shortcomings of functionalism, even of the more "cautious" functionalism which he identifies with Parsons (1951), Merton (1957) and Davis (1959), result from "looking at social structure as the 'backbone' of society, and considering structural analysis in social science as analogous to anatomy or morphology in biology." One result of this classificatory mode is a tendency toward the fallacy of misplaced concreteness, toward the reification of what "is" (Buckley, 1967). GST does not reject conventional functionalism, however, but recognizes it as a contribution limited by unnecessarily rigid categories. GST is *holistic*; it begins with the concept of *organization*—not of parts which happen to be related, but of relationships which may be studied by examining relevant subfields.

In recent decades, following the work of logicians such as Peirce (1934), Dewey (1938) and Whitehead (1929), and scientists such as Weiner (1948) and Von Bertalanffy (1968), it has been generally conceded that the older classificatory logic must give way to some form of *processual* logic. Older frameworks have begun with theoretical entities, and then tried to relate them by a categorical logic; GST begins with a processual conception of reality as

consisting fundamentally of relationships among relationships, as illustrated in the concept of "gravity" as used in modern physics. The term "gravity" does not describe an entity at all. There is no such "thing" as gravity. It is not even a force. Gravity is a *set of relationships*. To think of these relationships as entities is to fall into reification. GST recognizes "social facts," but in the same sense that gravity is a fact. This means, for example, that those sociological perspectives which treat social life as a continuous "negotiation of reality" are generally more congenial to GST than is the older notion of a specific set of norms systematically derived from a more abstract set of values. The GST approach demands that sociologists develop the logic of relationships and conceptualize social reality in relational terms.

Conflict theory seems to be most powerful when it treats social reality in relational terms. This is where Marx is often misunderstood. As Ollman (1971:15–25) has shown, Marx's conception of reality follows the tradition of the "philosophy of internal relations":

The relation is the irreducible minimum for all units in Marx's conception of social reality. This is really the nub of our difficulty in understanding Marxism, whose subject matter is not simply society conceived of 'relationally'. Capital, labor, value, commodity, etc., are all grasped as relations. . .

Thus capital in a situation where it functioned as interest would be called 'interest,' and vice versa. However, a change in function only results in a new name . . . if the original factor is actually conceived to be what it is now functioning as . . .

Generally speaking, we understand why Marx has used a particular name to the extent that we are able to grasp the function referred to . . .

What emerges from the interpretation is that the problem Marx faces in his exposition is *not* how to link separate parts but how to individuate instrumental units in a social whole which finds expression everywhere. (emphasis in original)

It is important to note that the term "function" is applicable only in the mathematical sense—not in the more usual sense of "consequence" or "purpose." A variety of illustrations may be

drawn from statistics, which deals not with the "parts" (e.g., individuals), but with relationships in the aggregate. A correlation coefficient is a relationship; an intercorrelation is a relationship between relationships. Factor analysis treats reality in relational terms. The problem of "massaging" sociological data is a problem of "how to individuate instrumental units in a social whole which finds expression everywhere" (Ollman, 1971:25).

Marx was quite familiar with the philosophy of internal relations, especially as it had been developed in Hegelian dialectics; in fact, the idea goes back to the ancient Greek philosophers. Its most recent expressions include the formulations of philosophers such as Dewey (1938) and Whitehead (1929), and the work of the biologists, physicists, engineers and others who have constructed GST as a way of dealing with practical scientific problems.

What is the approach to a reality consisting of shifting relationships? Those who continue to adhere strictly to Aristotelian logic tend to fall into one of two different traps of categorical logic. This can be illustrated by the running battle over the definition of "deviance." Deviance is not some sort of entity inherent in the act itself as the realists contend. But does this necessarily lead to the nominalistic position in which deviance is merely a label? In GST, the term is defined by examining various relationships and relational properties—not by imputing certain structural properties to the "deviant" or to those who are "labeling" him. Neither functionalism nor conflict theory has grasped this logic; the former tends to the errors of realism and the latter to the errors of nominalism.

In research on Mexican medicine hucksters (Simoni and Ball, 1975), close attention to actual empirical *processes* has made it apparent that although society treats the medicine huckster as a "deviant" and a "marginal man," hucksterism is better described as an "interstitial" role mediating between subsystems which are in a state of latent conflict. The medicine huckster is a colorful character who works the village market circuits of the Mexican hinterland, selling

medicinally worthless products by a remarkably effective sales pitch which captivates his audiences. His operations are illegal, yet he is allowed to work unmolested. While the concept of marginality implies a social system with certain deviants to be found along the outer edges, the concept of the interstitial role emphasizes that what is called *the* system is comprised, in this empirical instance, of a variety of differentiated social fields in shifting relationships to one another. What happens in the space between dominant fields (e.g., the development of medicine hucksterism) is a function of the relationship between the fields (in this case, the historically shifting relationship between the health care establishment on the one hand and the mass of the poor on the other). Marx (1906:784–848) has treated peddlers, vagabonds and "nomad workers" in such terms. A GST approach suggests that medicine hucksterism reduces pressure on the health care establishment due to rising expectations among the Mexican poor. Functionalism might emphasize that the medicine huckster is therefore "functional" to Mexican society. Conflict theory might describe him as another "opiate for the masses," a social technique employed by the powerful against the exploited peasantry. GST can combine these two perspectives, acknowledging the partial truth contained in each. The result is neither an implicit acceptance of the status quo nor an explicit call for social revolution; rather it is an exploration of various ways in which the obvious effectiveness of the medicine huckster might be directed into more socially useful channels. This approach has proved practical enough to suggest means by which effective huckster techniques might be employed to enhance the communication of badly needed health information to the Mexican poor, and to open up communication between the poor and the traditionally aloof establishment (Simoni and Ball, 1975).

Beyond Reductionism

According to GST, the various systems sociologists are concerned with (e.g.,

biological, psychological, or sociocultural) form a hierarchy of multiple levels, each level emerging from the previous. GST supports Spencer's notion that evolution proceeds from systems of "incoherent homogeneity" to systems of "coherent heterogeneity." Systems at advanced hierarchical levels achieve increased flexibility (wider "freedom") through the process of "complexification": increasingly sophisticated and integrative feedback loops lead to greater and greater internal differentiation. This means that although systems higher in the hierarchy are based upon those beneath them, they cannot be reduced to lower levels, because the principles of organization have become different. A sociocultural system, for example, is bound together by more complex relationships than is the material system upon which it rests. If we conceptualize society in hierarchical terms, with the symbol systems of religion, art and even law evolving from the economic level, GST tells us that (a) these "superstructures" are *based* upon the economic subsystem, but (b) they cannot be *reduced* to that level without a loss of understanding. Do the different aspects of capitalism, for example, follow *logically* from one another? Marx has disclosed some of the major internal contradictions which make capitalism "illogical" in any formal sense. GST allows us to make use of such conflict theories as Marxism without succumbing to their limitations, which are often reductionistic. GST might therefore accept certain formulations based upon a Marxist approach, but it would reject a strict economic determinism even as it would reject a psychologistic reductionism. The challenge is to explore the ways in which systemic processes at one level build upon different but related processes at another. We must spell out the "relationships" and the "differences" in rigorous terms which can be replicated in various empirical contexts. This is easier to propose, of course, than it is to accomplish.

Beyond Dualism

In an attempt to overcome our persistent metaphysical dualism, Laszlo

(1972:119) has provided a logically powerful explanation of the manner in which so-called "physical events" and "mental events" can be systematically related by GST. He describes a method of searching for their common features. As a practical framework which succeeds in escaping metaphysical dualism, GST may offer us one basis for a more systematic social psychology.

The methodological implications of any rejection of dualism are immense. GST insists that consciousness, having emerged as one aspect of the evolutionary process, has an affinity for the world precisely because it is a part of it; society is understandable precisely because it has been constructed by and is being constantly modified by the very beings which seek to understand it. This is not some mysterious phenomenological sensitivity. General systems theorists assume a stance of *radical empiricism* in the Jamesian sense. They "live in the experience," but also attempt to describe and measure it. The phenomenological impressionist, by taking only the first course, is rarely able to apply his personalized data to change the reality he encounters. The pure positivist, on the other hand, holds reality at a distance, contenting himself with reading his instruments. GST aims at the systematic translation of experience into communicable models. The individual and society are treated equally, not as separate entities but as mutually constitutive fields, related through various "feedback" processes. This is not a revolutionary idea; the potential contribution of GST lies in its comprehensive attention to a wide variety of systems and its attempt to draw analogies which may clarify the processes operating in one system by reference to another.

The connection between GST and the tradition of symbolic interactionism is now obvious. Shibutani (1968:330-331) has applied the GST perspective in a way which illuminates the more "cybernetic" Meadian concept of the act as an organized whole, pointing out that social action "is seen not as response to stimulation, as relief from tension, nor as the accomplishment of symbolized intent; it is something that is *constructed in a succes-*

sion of self-correcting adjustments to changing life conditions" (emphasis in original). As he also emphasizes, the relative openness of the process means that the same final action may result from different initial conditions in different ways, according to the GST principle of *equifinality*. Shibutani (1968:333) goes on to stress that "the key to all such self-correcting adjustments is *feedback*; all purposeful behavior requires negative feedback" (emphasis in original). The 'Me' is the self-image built up in feedback from others; the 'I' is the evoked response. These "inner experiences" do not take place in some introspective vacuum, for society is a complex net of feedback loops uniting individual members into a whole.

GST may also offer symbolic interactionism an opportunity to transcend its oft-criticized preoccupation with the purely interpersonal to the neglect of macrosociological factors through the concept of the image. Shibutani (1968:333) clarifies the Meadian appreciation of cognitive functions through his treatment of imagery, or cognitive mapping of information: "Any impulse that is not immediately consummated is transformed into an *image*, which serves as the basis for reflection" (emphasis in original). The economist and GST advocate Boulding (1961:14), has employed the concept of "imaging" at the societal level, in a way which ties together sociocultural and interpersonal dynamics. He draws heavily upon information theory, which has contributed a great deal to GST. In place of the positivist conception of social facts as immovable entities, he employs the concept of information flow, emphasizing that many so-called social facts are "only messages filtered through a changeable value system" (Boulding, 1961:14). Culture itself is treated largely in terms of such images. The parallel with Shibutani is clear. "Culture" is conceived of as the "image" of the society, the collective information by which the society attempts to organize itself.

Beyond Linearity

GST does not follow a linear logic; it therefore denies *both* idealistic formalism

and materialistic determinism. Those who lean toward idealistic formalism often see "normal" social action as consisting entirely of purposive, ends-means behavior. One influential example may be seen in Parsons (1937), where the "unit act" is conceptualized as if it were merely a "means" to realize a priori cultural ends called "values". The Meadian approach, especially as clarified by GST, highlights the inadequacies of the strictly unilinear ends-means view of idealistic formalism, emphasizing, among other things, the reciprocal relationship between individual and society.

The linearity of materialistic determinism is evident in the reflex arc (S-R) models of the Skinnerians, which treat social action as a set of conditioned responses. Recent work with the GST concept of "biofeedback" has called into question the strictly unilinear behaviorist position, demonstrating that "states of consciousness" can actually control what were once regarded as involuntary organic processes. This is not to say that people do not engage in instrumental actions, nor is it to say that we are not also creatures of habit. The problem is to find the terms which will clarify the relationship between these complementary processes.

The linearity of conventional sociological analysis is clearly illustrated in Merton's (1957) classic definition of social order in terms of institutionalized means-ends. Linear logic makes it almost inevitable that social disorganization, or anomie, will be treated as means-ends discontinuity. Social problems are then defined in terms of departure from this purely abstract construction. But much of the complexity of social change is reduced to the problem of anomie.

GST tries to use empirical research to develop formulations which reflect the processes of change as clearly as those of stability. GST has shown that most empirically normal systems follow a naturalistic logic of *multilinear discontinuity* rather than a formalistic logic of linear continuity (Boulding, 1956). The equifinality principle referred to above states that the goal may remain relatively constant while the means shift in response to internal and external feedback on developmental pro-

gress. For example, there is a genetic code for the development of an organism, but in actual operation this plan shifts *within certain limits* so as to accomplish essentially the same end in the face of varying environmental conditions. Another empirically common pattern is one in which the means remain relatively stable while the goals shift. Through the concept of displacement, this process has already achieved considerable importance in organizational theory. It can be described even more generally, however, in terms of the GST process of *equistasis*. Merton has done a great deal to clarify the problems which arise when institutionalized means are not adequate to permit realization of internalized cultural goals, but research which is restricted by Mertonian premises is likely to treat both equifinality and equistasis as disorganization. Research which begins from a more comprehensive GST framework may be more alert to the variety of organizational and relational possibilities and to the empirical factors surrounding each of them.

Beyond Homeostasis

The GST approach may be further clarified by turning again to conventional functionalism, which has contributed a great deal to GST by its insistence on the importance of the "system" concept. I will consider the most common criticism of functionalist theory: the limitations of an equilibrium concept which cannot account for such obvious empirical facts as the existence of internal conflict, deepening malintegration, maladaptive reactions to external change, and revolution (van den Berghe, 1963:697). From Malinowski's (1945) position, functionalism has developed a perspective much like Weiner's (1948) cybernetics. Weiner's use of the feedback concept has made an immense contribution to the resolution of many problems resulting from the traditional unilinear, causal analysis. He saw cybernetic processes as self-stabilizing, controlled by error-reducing *negative* feedback (Laszlo, 1972:38). The thermostat is the most frequently cited example of a control mechanism which keeps a flexible system in "dynamic equi-

librium" between designed tolerance limits: an increase or decrease in temperature provides the *negative* feedback. In functionalist theory it is value consensus which provides the *negative* feedback which maintains a changing society in dynamic equilibrium. This dynamic equilibrium is called *homeostasis*.

According to GST, it is not that functionalism has been in error, but that it has not gone far enough. If the collapse of the controlling *negative* feedback were the only source of social change, all change would result in increasing chaos. However, the actual empirical processes of evolution involve not only *negative* feedback loops which operate to restore the "natural order," but also *positive* feedback loops which amplify "deviant" tendencies. Much of the strength of conflict theory lies in its appreciation of these processes. The problem is that conflict theory has neglected sources of legitimate integration and has generally failed to see the *reciprocity* of conflicting relationships (see the discussion of victimology below). A system characterized by both positive and negative feedback is in a state of *homeorhesis*. Positive feedback changes the system, while negative feedback provides the integration necessary to maintain it. Analogies drawn from biological systems may clarify the nature of positive feedback.

In the empirical relationships between subsystems and suprasystems, certain types of biological environments favor certain types of mutants, which in turn produce changes in their environments. The phenomenon of "secondary deviation" (Lemert, 1951:77), which has become the basis of labeling theory, is such a process of positive feedback: the same environment which has fostered a variety of behavioral variations reacts to certain of them with counterproductive severity, thereby fostering a pattern of further compensating variations, while reducing the effectiveness of its own negative feedback as a constraining factor. Subsystem-to-subsystem relationships often reflect a mutually positive feedback process of escalation, as when an increase in the protective capacity of one species fosters increased predatory capacities in another,

producing a spiral of further protective capacities in the first. (A sociocultural analogy may be found in the mathematics of arms races, in which the moves of each international subsystem—i.e., nation-state—provoke counter moves by others [Richardson, 1960].) Element-to-element relationships within a particular subsystem (e.g., within a species) often develop processes by which mates of certain types are favored, fostering offspring with particular characteristics. All of these are “deviation-amplification” processes which propel change by way of *positive* feedback loops (Maruyama, 1963).

Of course, neither stability nor change is to be posited as desirable in itself. GST helps us to distinguish between the *power* of a particular process and its *result* in a way which illuminates both conventional functionalism and traditional conflict theory. A particular process is not necessarily functional merely because it is powerful. Actually, power may be directly correlated with dysfunctionality so that the more dysfunctional a process becomes, the more powerful it becomes; an example is the growth of cancerous tissue in an organism.

The field of “victimology,” which has developed since WW II, is another example of increased attention to the reciprocity of feedback relationships. Victimology suggests that crimes should not be viewed simply as actions springing from sources in the offender, but as “transactions” which can be explained in part by the behavior of the victim. In its early years, victimology often amounted to an attempt to depict the victim as the cause of his own victimization, sometimes to the extent of excusing the offender (Schafer, 1968). The reasoning remained essentially linear, except that what had been seen as cause came to be seen as effect, and vice versa. More recently, victimology has profited by a GST approach which breaks down the cause-effect distinction by emphasizing the process of interaction: a reciprocal flow of positive feedback between victim and offender, which often makes it difficult to decide just who should be charged with instigating the offense. The idea of transactions originated in the process theory of Dewey

and Bentley (Buckley, 1967). Through modifications of game theory (which has made major contributions to GST), it has become applicable to the therapy context as well as to the study of crime. Berne (1964) describes various “games people play” as patterns of sustained relationships built on *mutually* dysfunctional exploitation. These represent “negative-sum” games in which all players lose. There is little to be gained by defining these relationships as functional on the basis of their empirical endurance. At the same time, there is little to be gained by treating one side as the exploiter and the other as the exploited, for the exploited will often seek another similar relationship immediately after attaining freedom from the first. The nature of the feedback loops and the particular “information” being transmitted are problems which need to be approached through research.

Conclusion

GST combines a suitable epistemological position with a workable methodological strategy. It has drawn heavily on the prior contributions of other theoretical perspectives, while attempting to avoid their respective limitations. There are pitfalls to be avoided. A move toward a purely positivistic systems analysis would place undue limitations on GST, and that is one reason why the broader possibilities have been stressed in this review. The problem of system jargon, perennial to our discipline, must not be allowed to lead to more reinventions of the wheel, or to a drift into obscurantism. Above all, GST must prove itself in practice. What is needed more than anything else at this moment is the help of methodologically inclined sociologists who are more interested in learning from empirical problems than they are in producing elegant abstractions to be admired for their own sakes.

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LETTERS

To the Editor:

As an unemployed academic sociologist who is trying to practice and not perish, I found Morrissey and Steadman's article (TAS 12[Nov.]:154-162) of interest. I was surprised, however, to see no mention of an analogous situation in our sister discipline, psychology.

Before we re-invent the wheel on this issue, how about some research on the development and struggle of clinical psychology to co-exist as an equal to academic psychology within the APA?

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The fine article by Morrissey and Steadman on "Practice and Perish?" (TAS 12 [Nov.]: 154-162) deserves serious attention by all sociologists, whether in the academy, private

industry, or governmental agencies. The date is already late for acting on the important issues raised. However, the authors must be chided for employing a label that covertly perpetuates lower status. The term "nonacademic settings" suggests that the academy is, in some sense, more primary, and is thus a devaluing label. Labels such as "the new marketplace" or "practice settings" would be more neutral and, perhaps, more appropriate.

Parenthetically, I should like to note that some sociologists, including myself, enjoy being employed in the new marketplace, without feeling a sense of relative deprivation. This is an important point often overlooked by academic counterparts.

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MARXIST METHOD: STRUCTURAL CONSTRAINTS AND SOCIAL PRACTICE

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To understand Marx's unique approach to the empirical study of socioeconomic phenomena, it is necessary to delineate the roles played by both philosophic speculation and scientific inquiry in his work. In this paper, Marxism as both "philosophy" and "science" will be considered in some detail, in order to illuminate the strengths and shortcomings of each approach for the understanding of social change. The notion of praxis will be introduced as a way of bridging the two concerns. I shall argue that the various equations that comprise Marx's economic framework do not permit predictions concerning inevitable outcomes of capitalist economic production. This is because Marx's economic categories are at the same time reflections of social and political relationships; the values assigned to the former result in part from the latter, while themselves determining the limits within which the latter operate. To understand Marx as a scientist reducing capitalism to formal economic equations is to misread him as an economic determinist; to regard him as a philosopher focusing on reification and false consciousness is to ignore his central concern with necessary economic processes. Thus, I shall argue that Marx's work—as exemplified in Capital—constitutes a methodology capable of combining science's concern with lawful necessity, and philosophy's insistence on the possibilities for human freedom from external constraints.

The antimony between freedom and determinism can be viewed as strictly parallel to that between idealist philosophy and science as conventionally conceived. Science, according to notions prevalent among social scientists, seeks universal determinate laws embedded in the very structure of the universe itself. Such laws operate "behind our backs"—that is, they govern the universe whether or not we are aware of them. Freedom, according to such a formulation, is very limited: it is the ability to recognize the operation of those laws, impartially study their operation, and then govern our behavior accordingly. The laws of ballistics can never be abrogated, but, if we understand them, we can send a missile halfway around the world with unerring accuracy. In one sense Marx unquestionably saw himself as a scientist—a person concerned with understanding the "laws of motion" of capitalist economic production. But at the same time Marx could not accept a determinist version of science which ultimately—pushed to its logical limits—left no room for free, conscious activity. Such thinking would appear to be directly antithetical to the revolutionary project. Revolution is no simple mechanical activity of spelling out in accurate detail the complex of forces operating at a given

moment, constructing a course of action that respects those forces, and then pressing a button which starts the process towards its final outcome. For Marx, revolutionary struggle entailed not only a clear understanding of the social forces operating to produce social change, but it also involved continual choices, commitment, and an openness to the moment. It involved, in a word, freedom as well as necessity. Freedom is the traditional concern of idealist philosophy, which emphasizes intellectual emancipation through reflection and contemplation. It is not surprising that Marx spent his early years studying philosophy, that his first works were heavily "philosophical" in nature, and that he continually returned to "philosophic" themes throughout his life. Before understanding Marx as a scientist, it is necessary to understand the significance of philosophy for Marxist thought.

Marxism as Critical Theory

For Hegel, the purpose of dialectical thought was to take dogmatic notions about reality and render them transparent. Freedom, for Hegel, was inward: it resulted from a certain kind of contemplation which Hegel termed Reason. In the

chapter on "sense-certainty" in the *Phenomenology*, Hegel (1967:149-160) argued that the immediate sensual perceptions, which seem "to be the truest, the most authentic knowledge," the "bare fact of certainty," are really the "abstractest and poorest kind of truth." This is because both subject and object are continually changing, and—appearances to the contrary—exist only in relation to one another. The only real sense-certainty, as Hume demonstrated, is a congeries of unconnected point-instants: it is through mental processes that we secure the reality of an object in time and space.

Hence, the immediate appears real to the senses, but it is not. The radical implications of this position account for Hegel's appeal to Marx and to the circle of left-Hegelians who surrounded him, as well as to critical theorists today. In the chapter on "Commodities" in volume I of *Capital*, Marx sought to demonstrate how something as seemingly real and tangible as a commodity is actually something quite different from what it appears:

A commodity appears, at first sight, a very trivial thing, and easily understood. Its analysis shows that it is, in reality, a very queer thing, abounding in metaphysical subtleties and theological niceties.

A commodity is . . . a mysterious thing, simply because in it the social character of men's labour appears to them as an objective character stamped upon the product of that labour; because the relation of the producers to the sum total of their own labour is presented to them as a social relation, existing not between themselves, but between the products of their labour. . . . There it is a definite social relation between men, that assumes, in their eyes, the fantastic form of a relation between things.

This Fetishism of commodities has its origin . . . in the peculiar social character of the labour that produces them. (Marx, 1967: 71-72)

What Marx meant by this last sentence is that the apparent social relationship between two inanimate objects (the fact that two commodities exchange for one another) is due in reality to the fact that both embody specific amounts of human labor time, and hence reflect a particular social organization of production. The three volumes of *Capital* begin with the

chapter on commodities; the commodity is the immediate, "sense-certain" experience which we all take for granted. Using the commodity as a foil, Marx then sought to unravel the many threads that constitute the relations of capitalism; in so doing he demonstrated that simple commodity exchange presupposes a complex set of social, political, and economic conditions which had been quite invisible in the beginning.

Capital thus moves simultaneously on two levels. On one level, it is a scientific analysis, in Marx's terms, of capitalist economic production—a showing forth of underlying relationships and the special types of laws that govern them. On a second level, *Capital* is a critique of taken-for-granted thought—primarily of the categories of classical political economics, but also of popular notions of economic organization. *Capital* is appropriately subtitled "A Critique of Political Economy." It thus serves an important didactic purpose: it breaks through the veils of reification whereby people bestow naturalistic qualities on a world they themselves actually produce, as in the "mist-enveloped regions of the religious world [where] the productions of the human brain appear as independent beings endowed with life, and entering into relations both with one another and the human race" (Marx, 1967:217).

Critique, then, involves dissolving the seeming facticity of the conceptual categories that underlie our perceptions of the world—rendering "facts" transparent. To achieve such transparency, a "fact" must be situated within a concrete sociohistorical totality. Only by locating the commodity-form within capitalist economic production can its real nature be understood, and its seeming facticity thereby dissolved. The strength of critical theory is its unremitting emphasis on self-conscious human activity as the key to political emancipation. Reification—false consciousness—is the central concern; no automatic progression of society through inevitable stages of transition to a communist utopia is assumed, since an understanding of society is viewed as a necessary condition for its revolutionary transformation. But a weakness of critical

theory has been its excessive preoccupation with philosophical issues, to the neglect of the empirical study of the structural conditions Marx believed to contain the possibilities for social change. Marx believed such study would entail a "scientific" approach to political economy.

Marxism as Science

Marx's desire to develop a unified science of man and nature was expressed as early as 1844, when he wrote that "natural science will in time incorporate into itself the science of man, just as the science of man will incorporate into itself natural science: there will be *one* science" (Marx, 1964:143). This "orthodox scientific position identifies all possible knowledge with scientific knowledge (Habermas, 1971:4). While Marx undertook no systematic study of the natural sciences (that task fell to Engels), it is clear that he had a positivist's view of their procedures, and that, furthermore, he felt them to be an appropriate model for his own emerging science. In the afterword to the second German edition of *Capital*, published in 1873, Marx approvingly quoted a Russian reviewer, who, discussing Marx's method, observed that

... Marx only troubles himself about one thing: to show by rigid scientific investigation, the necessity of determinate orders of social conditions, and to establish as impartially as possible, the facts that serve him for fundamental starting points. For this it is quite enough if he proves, at the same time, both the necessity of the present order of things, and the necessity of another order into which the first must inevitably pass over; and this is all the same, whether men believe it or do not believe it, whether they are conscious or unconscious of it. Marx treats the social movement as a process of natural history, governed by laws not only independent of human will, but rather, on the contrary, determining that will, consciousness, and intelligence . . . in his opinion, every historical period has laws of its own. . . . As soon as society has outlived a given period of development, and is passing over from one given stage to another, it begins to be subject also to other laws. (Marx, 1967:18, emphasis added; see also pp. 8-9)

"What else," Marx asked, "is he [the re-

viewer] picturing but the dialectical method?" (p. 19)

As suggested in the quotation from the Russian reviewer, Marx moderated his scientific position only to the extent that he at times argued that economic laws are historically specific, that is, different laws apply to different historical periods. Thus, in criticizing Malthus in Volume I of *Capital*, Marx observed that "every special historic mode of production has its own special laws of population, historically valid within its limits alone" (Marx, 1967: 632). According to some who view Marx as a scientist, Marx's laws are more akin to those of biology than those of physics and chemistry, inasmuch as the latter "apply across the board in all places and at all times" whereas the former are seen as analogous to "historically specific laws" in that they apply to each species separately (Szymanski, 1973:31).

In actual practice, whatever his self-understanding, Marx's *usage* of the dialectic differs significantly from the scientific mode suggested in these quotations. Marx's methodological understanding was limited by his lack of concentrated attention to methodological issues, as well as his limited knowledge of the practice of natural science. Marx failed to come to grips with the differing requirements for the study of social and physical phenomena (see Habermas, 1971: esp. pp. 25-42); indeed, the debate within European scholarship over the two "sciences" occurred after his death. Nonetheless, he did seek to bridge his two concerns—science and philosophy—throughout his writing; and his attempt is best understood by utilizing the notion of *praxis*, which I shall now consider.

PRAXIS: The Dialectic of Subject and Object

Marx, particularly in his earlier writings but in *Capital* as well, regarded man as a world-producing creature: that which distinguishes man from other animals is his ability to erect a project in imagination and subsequently realize it in practice. In achieving such a project man is constrained by the external conditions he encounters, including the limits of his

knowledge; furthermore, those conditions (as well as his knowledge) are altered in the very process of realizing the project. Thus, Marx conceived of the labor *process* as an ongoing reciprocal relationship between objectification and reappropriation, with both theoretical and practical moments internally related. Human labor is thus conceived, ideally, as *praxis*. As Colletti (1971:84–85) observes, both causality and teleology are ideally operative according to Marx's conceptualization: *causality* because within ascertainable limits actions have determinate consequences, and *teleology* because action is consciously goal-oriented and therefore intentional effects may precede and govern efficient causes. Marx's entire work of criticism can be regarded as an attempt to demonstrate how the teleological aspects of human activity have been lost through the reification of the social world (and the consequent congealing of its laws). At the level of critique, it was Marx's intention to restore man's understanding of his potential role in the world, and thereby initiate the revolutionary project.

What distinguishes Marxism from positivist social science is its ability to move simultaneously on two levels: it formulates the laws of the "natural history" of capitalist economic organization, and at the same time demonstrates the ideological (i.e., historically situated) character of those laws so that they might be repealed by self-conscious workers organized collectively in their own interests. Criticism is not enough, nor is a theoretical understanding of how the laws of political economy operate and what they portend for the future of the capitalist economy. Nor is blind struggle; history does not move automatically towards socialism or any other predetermined end. Early in his writing Marx recognized that both critical awareness and scientific understanding were necessary to guide radical social change—as well as organized revolutionary activity. Science, criticism, organized class struggle—all are required, as Marx noted in 1843 when he wrote:

It is clear that the arm of criticism cannot replace the criticism of arms. Material force can only be overthrown by material force; but a theory itself becomes a material force

when it has seized the masses. (From *Critique of Hegel's 'Philosophy of Right,'* in 1972a:18)

Marx used "science" in a very restricted sense: his science was not the predictive science of physics or chemistry. The future cannot be predicted; rather, in Sartre's words, it is "a project to be accomplished" (Sartre, 1971:115). Our knowledge of the future occurs only through activity oriented towards realizing that future; the future itself becomes concrete to us to the extent that we participate in shaping it (Lukács, 1971:11). Sartre captures succinctly the meaning of praxis, when he observes that "what is essential is not that man is made, but that he makes that which made him" (Sartre, 1971:115).

Marx himself distinguished natural and social history when he echoed Vico in observing that "*human history differs from natural history in this, that we have made the former, but not the latter*" (Marx, 1967:372; emphasis added). Later in the same passage, Marx noted that the "abstract materialism of natural science . . . excludes history and its process"; this he regarded as a "weak point," which becomes evident whenever natural scientists "venture beyond the bounds of their own specialty" (373). Human history, for Marx, thus has a double nature: it is at once a project of human activity and a constraint on that activity, a social construct and an apparently natural condition of such construction.

Men make their own history, but they do not make it just as they please; they do not make it under circumstances chosen by themselves, but under circumstances directly found, given, and transmitted from the past. The tradition of all the dead generations weighs like a nightmare on the brain of the living. (1972b:437)

Praxis is the union of theory and practice, the mutual transformation of subject and object, the precondition for reappropriating the social dimension of naturalized human history—these are the meanings of the term imparted by the Marxist tradition of critical theory. The utility of a "praxis orientation" for empirical social research will be demonstrated in the concluding

section of this paper, by referring to Marx's treatment of what he regarded as a fundamental "law" of capitalist economic production: the tendency of the overall rate of profit to decline over time, thereby producing unavoidable economic crises. Before proceeding, however, it will be necessary to briefly introduce some of the basic terms of Marxist economic analysis.

The "Law" of the Falling Rate of Profit¹

Marx analyzed the value of commodity production in terms of three elements: (1) *constant capital* (C), the value of the means of production used up during the production process (primarily the depreciated value of machines, buildings, and raw materials); (2) *variable capital* (V), the value of the labor-power applied to the production process (primarily the wage bill); and (3) *surplus value* (S), the value of unpaid labor appropriated by the capitalist during the production process (workers' labor time beyond that which is "socially necessary" to sustain the standard of living of the working class). Surplus value is the key to capitalist economic production, for it is the source of all profits, including (and most importantly) those which are reinvested in enhanced productive capacity (capital accumulation). During the period of competitive capitalism (with which Marx was primarily concerned), individual capitalists were under continual economic pressure to increase the efficiency of production—to produce commodities at lower unit costs. While this could be achieved by economizing on either of the two principal component costs of production—constant or variable capital—Marx believed that in the long

run the key to lowering production costs was mechanization, which meant increasing C relative to V (1967: 265; 1972b: 186). Thus, driven by the economic imperative to undersell competitors in order to remain afloat, individual capitalists would be forced to substitute increasingly efficient machines for human labor. Throughout the economy, therefore, there is a long-run tendency for what Marx termed the "organic composition of capital" to rise, as denoted by the symbol Q, where $Q = C/(C+V)$.² This tendency, in turn, made it possible for the remaining workers to produce ever-larger quantities of goods with ever-decreasing labor-time; as a consequence there is a parallel tendency for the rate of surplus value (S')—defined as the ratio of unpaid to paid labor time (S/V)—to rise as well.

How do these tendencies affect the overall rate of profit in capitalist economic production? Marx defines the rate of profit (P) as the ratio of surplus value to total capital advanced, or

$$(1) P = S/(C+V)$$

from which it follows algebraically that the rate of profit can be decomposed into two terms comprised of the rate of surplus value and the organic composition of capital:

$$(2) P = S'(1-Q) \text{ where } S' = S/V \text{ and } Q = C/(C+V)$$

Marx argued that a rising organic composition was the hallmark of capitalist production (1967:449; Marx and Engels, 1972:338). It follows that as Q approaches 1, (1-Q) approaches 0, with a consequent depressing effect on the overall rate of profit. On the other hand, inasmuch as the reason for mechanization in the first place is to increase S, the downward pressure on profitability resulting from rising Q will be partially offset: to the extent that S' rises as Q simultaneously rises, the value of P is indeterminate. Marx was well aware of these considerations, but argued on logical grounds that as the organic

¹ Marx's theory of the falling rate of profit is only one aspect of his overall theory of the crises of capitalist production. The other principal aspects have to do with realization crises (see especially Sweezy, 1968)—those resulting from the overproduction of capital and commodities relative to demand. Recent reformulations of Marxist economic theory have identified further sources of crisis appropriate to the phase of monopoly capitalism; these include the theory of the profit squeeze (Glyn and Sutcliffe, 1971) and theories focusing on the role of the state in absorbing surplus and stimulating accumulation (e.g., O'Connor, 1973; Mattick, 1969; Baran and Sweezy, 1966; Offe, 1973; Altwater, 1973).

² Marx generally spoke of the proportion or ratio of C:V; the organic composition of capital is expressed by some writers as C/V (e.g., Mattick, 1969; Mandel, 1968). I shall follow Sweezy's (1968) usage, which defines the organic composition as the ratio of constant capital to total capital advanced.

composition reaches a high level, additional increases in productivity (hence *S'*) are inadequate as a strategy to maintain profitability (1967:247; see also 1973:338–340 for a crude mathematical ‘proof’).³ Since capitalism is production for profit, once the overall rate of profit (or at least that obtaining in key economic sectors) drops below some minimally acceptable level, production ceases: factories close down, and an economic crisis ensues. The profit-maximizing strategy of individual capitalists has resulted in a profitability crisis for the class of capitalists as a whole. This is, for Marx, a structural imperative of capitalist economic production.⁴ Yet despite the compelling nature of such an imperative, its actual working-out depends on concrete sociohistorical circumstances: the declining rate of profit is a *tendency* that manifests itself within and through class struggle, and not a *law* which operates automatically outside of human practice.

Utility of the Praxis Orientation

I believe the distinction between “tendency” and “law” is the central feature of interest in Marx’s theory and method. I have argued that Marx sought to avoid both the determinism of a completely materialist science, and the voluntarism of idealist philosophy. He achieved this by conceptualizing the material conditions of action as embedded within interrelated

social, economic, and political structures, while regarding human action itself as capable of modifying the underlying structures (and hence the conditions of future action). In the example of the declining rate of profit during the period of competitive capitalism, the principal structures of analytic interest are economic. Marx did not treat these structures as obeying universal laws that lead to precisely predictable outcomes: he was not developing a social physics. Rather, he advanced the position that while the economic structures of competitive capitalism impose certain requirements on economic actors, those requirements lead to contradictory outcomes which undermine the structures themselves, creating opportunities for conscious political action. Marx’s treatment of economic “laws” or “tendencies” can be characterized as follows:

(A) Laws constitute theoretical statements concerning the structures of principal interest in understanding capitalist economic production. When expressed formally, as in equation (2), they draw attention to predictable outcomes (such as a declining rate of profit) given certain conditions (such as a rising organic composition which outstrips the rising rate of surplus value).

(B) These conditions are theoretically conceived as socially accomplished rather than naturalistically given (although they may be perceived as the latter by economic actors). Thus, both a rising organic composition and a rising rate of surplus value result from the behavior of capitalists (who strive to economize labor) and labor (which organizes to oppose and reverse such efforts). However, such behavior is by no means freely chosen; rather, it is more-or-less restricted and hence more-or-less determined in the short run. The restrictions stem from the “givens” of a particular mode of production (for example, capitalists must remain competitive to survive, given the existence of a market economy; this in turn entails strong pressures to introduce technologies which economize the cost of production, producing a rising organic composition; and so on)—they are thus experienced as social facts. The “more-or-less” quality of the restrictions reflects

³ See Wright (1975:37, footnote 7) and Yaffe (1973:202) for a mathematical demonstration that “as the organic composition of capital rises, the rate of profit becomes progressively less sensitive to changes in the rate of exploitation [i.e., surplus value]” (Wright, 1975:16). Marx also detailed a number of empirical influences which may for a time counteract the tendency of the organic composition to rise (see 1967:232–240), but these are judged insufficient in the long run to mitigate the overall process.

⁴ The crisis itself is an integral part of the dynamic of capitalist production. While it temporarily restores profitability through lowering the organic composition (see Yaffe, 1973:205–206 for an elaboration), it does so by altering the framework of production itself, through contributing to the centralization of capital: economic crises thus abet the transition from competitive to monopoly capitalism (Marx, 1967: 250–251; see Wright, 1975 for an excellent discussion of these processes).

the fact that during times of crisis, the range of freedom is greatly extended. For example, during crisis periods large and powerful capitalists are able to acquire the more marginal enterprises, thereby reducing competitive pressures through monopolization (Wright, 1975; Yaffe, 1973; Cogoy, 1973). Following a crisis, according to Marx, the range of action is again circumscribed—although the requirements of the new phase (e.g., monopoly capitalism) may involve different restrictions than those of the previous one (e.g., competitive capitalism). During periods of particularly acute crisis, a great many restrictions on action may be relaxed. Such times are revolutionary times, and a properly organized and politically conscious working class has the potential of totally abrogating the laws of capitalist economics (Marx believed) through the establishment of a planned, socialist economy.

(C) Within a mode of production, laws are conceived as depicting structured instabilities or contradictions. Capitalism—conceived ideally-typically by Marx as a closed economic system—entails contradictory elements which work to undermine the system despite the intentions (and, indeed, even awareness) of its ruling sectors. Thus, for example, the logic of production as I have described it implies an eventual decline in profitability for capitalists as a whole: rational, survival-oriented behavior on the part of individual capitalists spells long-term disaster for the capitalists as a class. (A similar argument can be made with regard to the Keynesian problem of insufficient aggregate demand, according to which the necessary economizing of labor-costs reduces the purchasing power of the working class, thereby producing tendencies towards overproduction and underconsumption; see, for example, Sweezy, 1968, 1974; Hodgson, 1974.) According to Marx, such contradictions are an inevitable feature of production in all class-based societies, that is, in all societies where surplus value is produced by one class and appropriated by another. Since one result of such contradictions is a persistent tendency towards recurrent economic crisis, the resolution of contradictions cannot be

predicted; final outcomes depend on the action taken by the principal classes involved in production.

(D) Thus, while Marx may express economic relationships in the form of seemingly lawful equations such as (2), the terms of these equations—S, V, and C—must be taken as denoting social relations rather than purely formal economic ones. In particular, C, V, and S are indicators of the degree of class consciousness and class struggle, and this is an important feature of their role in Marx's economic equations. Surplus value (S), for example, is the arena of the struggle between workers and capitalists (or their managerial representatives) over the length of the working day and the intensity of the labor process; the outcome of that struggle is ultimately the product of such historically unique circumstances as the degree of economic crisis or stability, the level of working-class consciousness and political mobilization, the possibilities for substituting inexpensive foreign labor through capital export, and the extent to which capital enjoys monopoly control over production. Variable capital (V) is the arena of struggle over wages and subsistence; its outcome similarly reflects the relative power of labor and capital at a particular conjuncture—the extent to which capital can “deliver the goods” cheaply while incorporating key elements of the labor movement, and the degree to which labor can press its claims for a higher standard of living in a unified fashion. Finally, constant capital (C) denotes the extensiveness of capitalist economic relations—the ability of capitalists to economize capital through appropriate technologies or foreign investment, the intensity of competition among capitalists, and the degree of class-conscious organization among the owners of large capital (for an elaboration of these examples, see Appelbaum, forthcoming).

The struggles over workers' share in output, the disposition of the surplus, and capitalists' control over the productive process; these are merely different aspects of the class struggle. That struggle is not purely economic, although it depends to a large extent on economic conditions, and affects those conditions most directly.

The class struggle, as seen by Marx, also moves at the political and cultural levels. The possibilities of delegitimation of the state and dereification of both popular and scientific culture flow from economic struggles, and shape those struggles. That is why "theory itself" becomes a material force when it has "seized the masses" (Marx, 1972a:18).

Conclusion

There is nothing automatic about the processes of social change. This is true of the broadest historical view (i.e., the "stage of societal development" often attributed to Marx), as well as of more historically-bounded economic "laws" (e.g., that of the falling rate of profit under capitalism). Changes in concrete societies occur within well-defined structural limits; those limits are given for capitalist forms, within Marxist political economy, by the hypothesized relationships among the parameters C, V, and S. But those limits can be changed, the relationships among the parameters can be altered, and the parameters themselves can change in value independently of their necessary connection within formal equations. This is because C, V, and S, while serving as economic parameters, were ultimately conceptualized by Marx as signifying social relations, of which the quantitative economic measures (hours of labor time, price) are merely surface indicators. Social relations can be altered, within bounds. Those bounds were, for Marx, first and foremost the structural conditions of economic production. The structural conditions generate problems (contradictions), while setting limits to the solution of those problems. The likelihood and efficacy of any economic solution depend, in large part, on the legitimacy accorded to the growing state intervention (with its mounting economic costs) and on political consciousness and class militancy in general. Crises of legitimation, dereification, class organization and struggle may grow out of adverse economic conditions (or equally adverse "solutions" to such conditions), but they are not reducible to economic factors. The future cannot be predicted from within a

Marxist framework. It can only be shaped and guided by adequate theoretical understanding, but it is ultimately dependent on the behavior and conscious understanding of organized social classes.

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... modern science is definitely committed to
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LINDSAY AND MARGENAU

Foundations of Physics

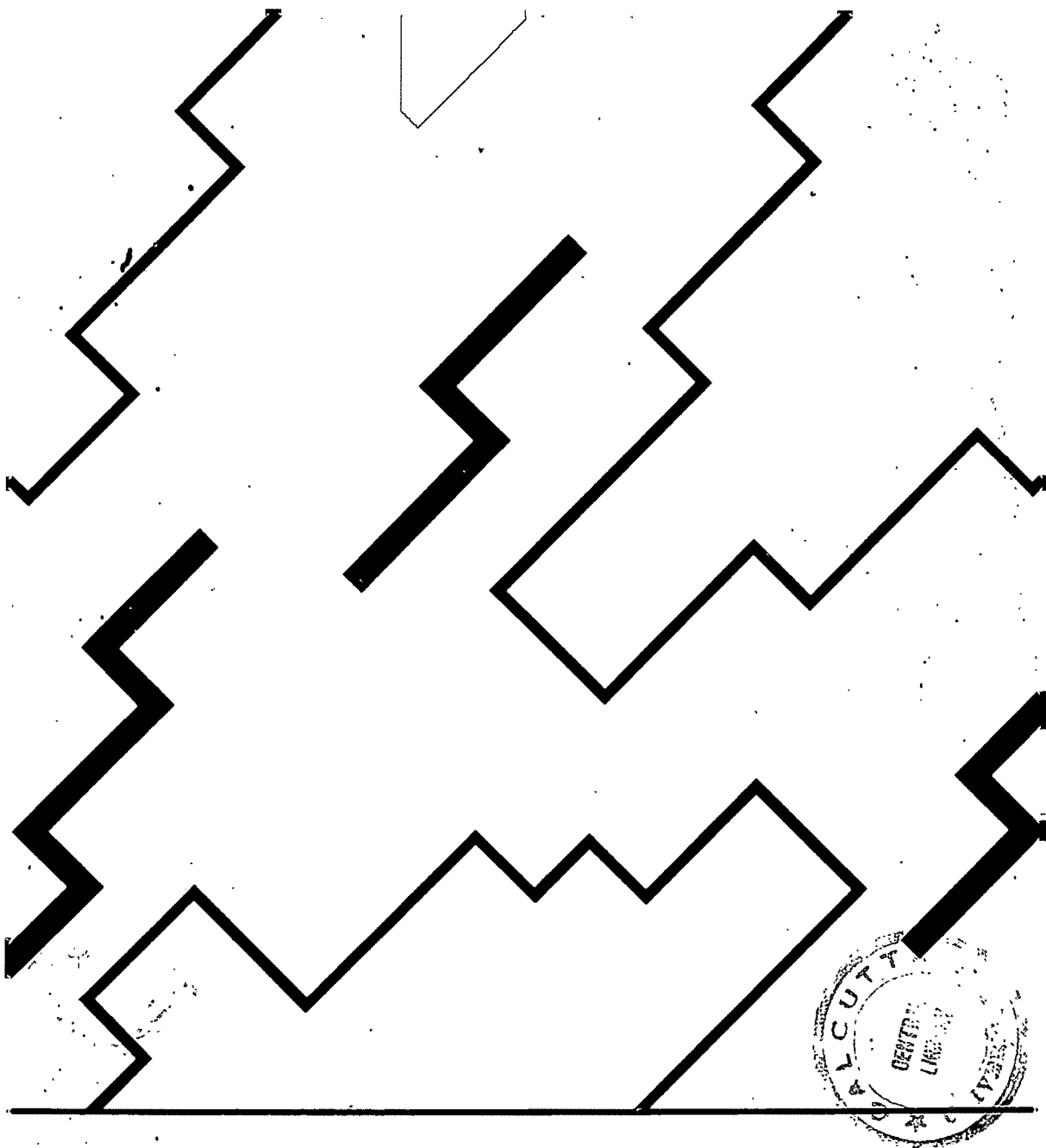
I am a message, they say,
 a code of chemicals,
 nothing more nor less.
 They tell me,
 those bleak men
 with basilisk eyes
 peering through black horn-rimmed beakers,
 that I was written out
 long ago,
 back when genes were jostling for place,
 queuing up until they finally froze
 into a yinning yang
 of four-letter words:
 a tortuous Tetragrammaton
 erupting with the next statement
 in the book of life.

But I gaze about me,
 agog
 at all this gorging conversation,
 this babble of a billion messages
 surging to utter self,
 and wonder what sign
 will mark the end of my sentence:
 comma,
 colon,
 or full-stop?

VITO SIGNORILE
 University of Windsor



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A NOTE FROM THE SPECIAL EDITOR

Annual reports from editors of ASA journals regularly inform us that sociologists are a productive lot; most sociological journals routinely receive more manuscripts than they could possibly publish. Letters accepting articles for publication just as regularly plead with authors to condense their presentations due to pressure for space in the journals. These observations, as well as the proliferation of journals over the past decade, clearly demonstrate that sociologists have more to say about their research than we have space for their findings. While *The American Sociologist* is not a research journal, this special issue on "Sociology and Complementary Disciplines" deals directly with sociological research that employs data and methods primarily found outside of sociology. As special editor of the issue, I fully anticipated facing the perennial editor's problem—too many worthy papers and not enough pages for them.

But I soon learned that my concern was unwarranted. The major problem I faced in compiling this issue was not a plethora of papers, but a lack of them. We advertised this special issue for over two years and received only ten manuscripts. I interpreted this situation as reflecting a limited sociological concern with interdisciplinary issues. Should I have concluded that sociologists have nothing to learn from other disciplines? If our colleagues are so unconcerned with issues of cross-disciplinary applications, why did I continue seeking papers? Perhaps I was devoting precious journal space to one of my own idiosyncratic predilections. Despite the limited number of submissions, I think that the papers we present in this issue confirm my original feeling that sociologists *can* look beyond their own traditional approaches and data and find valuable avenues of research not generally recognized. I hope *TAS* readers will agree.

The papers in this issue obviously do not cover the spectrum of possibilities. They fall primarily into two major areas of interdisciplinary relationships: the first, linguistics and sociology, is a relative newcomer to the literature; the second, history and sociology, has an extensive lineage. Our authors document problems of interdisciplinary research, and point out new directions that we would do well to consider. Heath's paper was a fortuitous addition to the set, because her discussion spans all three disciplines: history, linguistics and sociology. She argues that the study of language in its social context can be informed by the use of historical documents that codify language use norms. Eighteenth- and nineteenth-century etiquette books and conversation manuals provide data that sociologists of language should not ignore. As a stratification researcher, I find Heath's suggestions very useful for taking the sociological analysis of language beyond contemporary societies; her discussion suggests the value of historical documents on language use for studying issues such as changing patterns of social class and sex stratification.

Mariampolski and Hughes call our attention to another data source for historical sociologists. They outline both the research potential of historical personal documents, and the rigorous tests of reliability and validity that must be applied to these data. Personal documents are not a new data source for some of our colleagues, but I am sure many of us have rejected them (and similar qualitative sources) as data because of the potential problems identified here. Mariampolski and Hughes go beyond a recitation of potential pitfalls, and may lead some of us to reconsider.

The papers by Labov and Zaret deal

Continued on Cover 3

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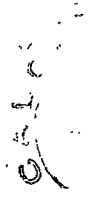
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SOCIAL HISTORY AND SOCIOLINGUISTICS*

SHIRLEY BRICE HEATH

University of Pennsylvania

The American Sociologist 1978, Vol. 13 (May): 84-92

This paper examines the relations between the data, methods, and interpretations of social history and sociology, with specific attention to the potential of sociolinguistics in the use of historical materials by sociologists. Data from conversation manuals, etiquette books, and grammars on introductions, greetings, and the regulation of conversational interaction, illustrate diachronic variation in the use of these formulaic expressions. The context of the egalitarian ethic of Jacksonian Democracy highlights a nineteenth-century shift in the use of politeness formulae by and to speakers of different social classes. These examples illustrate the need for an historically informed sociolinguistics.

Historical research, often invoked to show "how we got where we are," can also provide a perspective from which to compare current interpretations of behavior with interpretations of past societies. Historical data can add a dynamic focus to the search for improved approaches to describing and analyzing the context of current behaviors. Historians explore the interrelationship of people's actions and values through time, searching for patterns or themes of change. Students of current behavior can compare their analyses of forces of change to the historians' determinations of broad patterns of continuity and change which arise out of networks of social relations established in the course of time.

Social historians share emphases, methods, and data with sociologists. Questions raised by both history and sociology relate to the family, ethnic groups, social mobility, and shifting estimations of occupations. Quantitative analysis and a central concern with theoretical and empirical literature on *status*, its definition, identification, and measurement, link the two disciplines. The data of both include census reports, state and county records, opinion analyses, and documentary histories of institutions. In the study of many topics of mutual interest, such as urban and com-

munity life, and analyses of professions and institutions, the two disciplines have exchanged data and methods. However, on the topic of language and its use in various contexts, there is hardly any sharing of data, methods, and interpretations between historians and sociologists.

Toward an Historically Informed Sociolinguistics

Social historians have traditionally considered language and its changes in relation to uses and users the subject matter of literary critics, biographers, and occasional narrative historians. Language can be studied both for its internal structural system and for its relationships to external systems—social, political, and cultural. The latter focus best fits the purposes and sources of social historians. Intent on portraying the history of all sectors of society, social historians have drawn from quantitative sources (census data, wills, institutional records, patents issued, divorce records) and qualitative documents (diaries, letters, newspaper fillers and letters to the editor). Using these sources, historians have provided socio-historical treatments of the family (e.g., Hareven, 1977), the school (Tyack, 1974), the city (Demos, 1970), women (Cott, 1977), and Blacks (Guttman, 1976). Within their treatments of each of these broad subjects, historians have focused on highly specific aspects of ways of living: mother-child interactions, female-physician relations, and community-school interconnections. They have often cited primary materials describing, prescribing, and

* A slightly different version of the material in the politeness formulae section of this paper was presented with Charles A. Ferguson at the American Anthropological Association meetings, November, 1977. The assistance of Laura Nell Ford in collecting and analyzing nineteenth-century greetings and introductions is gratefully acknowledged.

evaluating language use, but they have rarely interpreted these sources on the issue of language use in specific interactions.

For sociologists, language provides a rich source of raw materials, the analysis of which can contribute to both theory and methodology. In analyzing social stratification, sociologists have pointed out the symbolic power of language as a reflection of social class position and as an indirect evaluation of the class of others. They have suggested inconsistencies in the use of different language forms in American society: some (such as the frequent use of first names) would seem to be consistent with the ideology of universal equality; others (such as greetings and introductions) more frequently emphasize class differences (Barber, 1957). However, little attention has been given to the codified sources which provide ideological support for these language forms; nor to possible correlations between changes in the content of these sources, and material conditions, political trends, etc. Historians interpreting etiquette and manners in different societies and periods have almost always stressed the role of these conventions in maintaining inequality (cf. Schlesinger, 1946; Lynes, 1963; Davis, 1975). Yet such interpretations have not resulted from content analysis of etiquette books or conversation manuals, quantification of users and uses (for example, library records, number of schools and grade levels involved in moral education curricula requiring etiquette books as texts), and correlations of these data with traditional sociodemographic materials. This type of investigation of the social structural and ideological connotations of codes of etiquette would seem particularly relevant to those sociologists who emphasize the part played by belief systems in history, and who view society primarily in terms of the interplay of group interests and ideas.

Sociolinguistic theories and methods provide a starting point for this kind of research. However, sociolinguists have neither used historical materials nor substantially related their findings to the thematic concerns of sociologists and social historians.

Sociolinguists are generally concerned with language in its social setting and ways in which its structure and functions are maintained, altered, or obliterated by specific aspects of social interactions. Sociolinguists examine many aspects of language as both macro and micro phenomena. Some attempt to determine rules of variability of a single speaker or a specific group in a particular context in order to provide data for comparative analysis (e.g., Labov, 1966). Conversational interactions and the performance of speech acts concern sociolinguists interested in theories of meaning. Both the content and the force of fundamental speech acts contribute to the knowledge of systems of human interaction in different settings (Goffman, 1963, 1967; Turner, 1974). Other sociolinguists more oriented to anthropological methods and concepts attempt to determine how the total round of living affects verbal and nonverbal communication in a specific culture or institutional setting (e.g., Gumperz and Hymes, 1964, 1972; Bauman and Sherzer, 1974). These sociolinguists ask such questions as how the language input of mothers affects children's speech; how children learn to communicate competently with different listeners, topics, and settings; how language varies in use between men and women, young and old.

In these varieties of sociolinguistic research, historical data, methods, and interpretations have been minimally used. The techniques of historiography and the explicit theories of historical analysis which enable historians to locate and evaluate appropriate data (see Dollar and Jensen, 1971) are not generally known to sociolinguists. Moreover, historical data are found in widely scattered, often inaccessible repositories, and many primary sources can be understood only in the context of historical events which may not even be mentioned in the primary data.

An historically informed sociolinguistics would incorporate social history with concerns and methods more in line with philology than with linguistics: use of historical texts and study of language in connection with various other cultural phenomena, such as religion, aesthetics, political shifts, etc. The use of sources which

have the potential of giving us the history not of human speech, but of humans speaking seems particularly appropriate to the current emphasis on context by both social historians and sociolinguists. In the United States, both social history and the sociology of language emphasize similar—at times almost parallel—concerns. Social historians have termed their field “the most ambitious form of history” (Zeldin, 1976) because it attempts “an approach to the entirety of the past” (Stearns, 1976). In their search for relationships which set social forms and trends, social historians have described and explained styles of life across sectors of society. They have been concerned with methods to “listen to the inarticulate” (Lemisch, 1969), to record the habits, values, and attitudes of those segments of society overlooked by historians, who have traditionally relied on sources produced by and for elite or middle class sectors.

Sociolinguists embrace an equally ambitious scope and similar subjects when they seek to study “language as it is used in everyday life” (Labov, 1972:xiii), to consider the daily uses of language across all sectors of society, and to elicit data from those whose estimations of correctness are not recorded in grammar books. Both groups value qualitative data: individuals talking about how and why they behaved as they did. Quantitative data supplement these normative judgments to show what it is individuals or groups did, in fact, do (cf. Rothman and Rothman, 1975; Fishman, Cooper, Ma *et al.*, 1971). Their subject matter is similar: work and educational settings, women, small group interactions, splinter groups, and ethnic associations. Both groups debate the relative merits of quantitative vs. antiquantitative positions, yet both increasingly move toward a combination of these approaches (Chirot, 1976).

Sociolinguists have been reluctant to use historical data because language data from the past is generally considered tainted with value judgments, representative of the ideals of communication rather than its realities.¹ The absence of scienti-

fically agreed upon ways of recording language so that data can be compared across cultures, social groups, or periods of time has caused sociolinguists to see the written materials accidentally preserved as unsuitable for analysis. Many of these limitations, however, can be met in part through the wide range of innovative methods in historical research which enables historians not only to locate and evaluate new sources, but also to bring different kinds of sources together to help test hypotheses (cf. Berkhofer, 1969; Lipset and Hofstadter, 1968). For example, sociolinguists ask questions about how today's children learn uses of varieties of language for specific topics, settings, and listeners, and how they come to evaluate different varieties and uses of language (e.g., Snow and Ferguson, 1977). Methods of historical analysis and local historical records enable sociolinguists to ask these questions about children of another era as well. Since the colonial period, local historical societies have been the repositories of records ranging from old textbooks with margin notes to diaries of school teachers, accounts of school events, and school board reports. When a single school district has preserved essays written by children in school, the teacher's diary, letters from parents, a play on language, and school board reports containing accounts of language preferences in schools, the sociolinguist can determine: (1) the content of language lessons, (2) attitudes of the teacher toward correct speech, (3) uses of language in written sources, ranging from essays to personal letters, (4) the similarities and differences between the writing of parents and that of children, (5) the language of public performances planned by students, and (6) the responses of school officials to what

and diachronic linguistics see language as something historical and as subject to change across time. However, both these fields focus on the description of a language's structural changes and laws or forces of change. Factors external to the language system are not systematically included. Most research on sociolinguistics has been synchronic in scope. Only recently has the potential of diachronic perceptions of language varieties and their uses in social contexts of various speech communities been recognized (Bodine, 1975; Bauman, 1974).

¹ To be sure, scholars of the history of language

children have learned about language.² In many ways, these data are similar to those collected in current ethnographic studies of community literacy and teacher-student interactions. However, historical data provide a breadth of coverage and means of examining consistency between language teaching and learning (as well as estimations by adults and children of the success of efforts to promote "correct language") lacking in many current studies.

Politeness Formulae

Another example of the usefulness of historical data is in the detailed sociolinguistic analysis of specific speech acts, such as politeness formulae. Every speech community has highly stereotyped verbal routines which are largely non-referential but are appropriate for use on certain identifiable occasions of communication. Such verbal routines include proverbs, thank you's, greetings, congratulations and others. Some of these occur frequently in everyday verbal interaction; for instance, greetings and introductions serve to facilitate the exchange of substantive messages, and in a variety of ways, help to carry on the "business" of the community. They may be studied from a number of quite different perspectives, ranging from a concern for their linguistic structure to the interpretation of their social functions (for a general treatment and references, see Ferguson 1976; Ibrahim *et al.*, 1976).

Certain politeness formulae, such as greetings and introductions, are basic to regulating interactions in which distances must be bridged, i.e., when two strangers meet, when an intimate meets an intimate after a distance of time, when two intimates meet in a situation which creates a need for social distancing, or in a situation which necessitates distancing between so-

cial roles (as in interactions between waitresses and customers). All of these bridging maneuvers accomplished by greetings and introductions require each person to reflect on his or her own social class and evaluate the social class of others. Acquisition of the rules for determining social standing and choosing appropriate politeness formulae for particular occasions, mixtures of social class, etc., may come from modeling by parents, elders, or societally approved instructors. However, in a society shifting from an emphasis on gentility by birth to the cultivation of civility by all willing to invest the individual effort, parents and elders cannot be trusted as models. Written sources, such as conversation manuals, grammars, and etiquette books, become the trusted sources of rules.³ These sources describe verbal routines and rules for when and how to use them in specific situations, with particular topics and listeners. Thus, ways of indicating one's own social stand-

³ In the reconstruction of any kind of social behavior involving moral or aesthetic judgments, great care must be exercised in interpreting the historical materials. The interpretations offered here are tentative ones drawn from a larger study, a social history of language in the nineteenth century. Data for this analysis were drawn from approximately 350 conversation manuals and etiquette books published in the United States between 1780 and 1900. There is no comprehensive bibliography of these sources and estimates of the total number are always highly speculative (Schlesinger, 1946). However, books in these categories were consulted in the following libraries: New York Public, Boston Athenaeum, Huntington, American Antiquarian Society, University of North Carolina (Chapel Hill), Library Company of Philadelphia, American Philosophical Society, Boston Public, and Library of Congress. Estimations of the popularity and readership of these sources were made through a variety of methods. For selected decades, quantitative analysis of the number of readers checking the book out of the Boston Athenaeum was possible. In a number of libraries in small towns, it was possible to determine number and social class of readers; for example, records necessary for this kind of analysis for specific decades are available at the Essex Institute in Salem, Massachusetts. School boards often used etiquette books in moral education curricula near the end of the nineteenth century; their records indicate the extent of use of these sources among readers in specific age groups. For the earlier decades of the century, advertisements in juvenile periodicals indicate which of these sources may have had the widest distribution among the book-buying (as opposed to book-borrowing) public.

² I am grateful to David Tyack for first pointing out the potential of such collections for the analysis of language in use and for identifying the collection of Oliver Applegate in the Library of the University of Oregon (see Tyack, 1966). For a development of the potential of materials such as these on the evolution of standards of correctness, see Heath (forthcoming).

ing and evaluating that of others become background information in these sources prescribing language use. Sociolinguistic concerns and social history data and interpretations come together in examining politeness formulae and their recommended uses.

A basic theoretical position (Gumperz and Herasimchuk, 1975:81) is reflected in these sources: that "in the analysis of face-to-face encounters . . . role, status, social identities and social relationships" are treated as communicative symbols. The meanings of these symbols depend on situated descriptions of speakers, their characteristics with respect to age, sex, social class, cultural background, and ideally, the intent of the interaction. Many conversation manuals, grammars, and etiquette books of the eighteenth and nineteenth centuries provide some or all of these requirements of situated descriptions. First, many are written for a specific readership—males, females, or children—which is carefully defined in the preface. Second, the manuals contextualize their rules or admonishments in discussions of generalized social norms; many books are written as conversations and include actual speech events recorded by the author to provide background for learning about language in use.

For example, the author of *Art of Good Manners, or Children's Etiquette* (Power, 1899), writes the book as a conversation with a young girl waiting outside the parlor while her mother entertains a guest. The author describes the scene, assumes the girl is not intruding for fear of not knowing how to advance, and guides the girl's steps through the interaction, providing behavior alternatives in response to the moves of the adults. Throughout the book, the child is cautioned to take cues from specific adult moves and to observe sequences of exchanges. For example, the child is advised to enter the room only after her mother and the guest have "established contact." The child should not speak when entering, but wait for an adult to say "How do you do?" The child is advised that she may answer either "very well, I thank you," or "not very well," as the case may be. At that point, the child is not to speak further until the adult does

so. If the adult does, it is most likely to be a sequence of questions. The child is not to answer each of these with either yes or no, but to provide variety in answers. To fill the space of the conversation, the child is not to carry on the conversation for both sides, as adults sometimes do, asking and answering: "How do you do, Miss Didley? I've been wanting to know you ever so long, mamma has spoken so much of you. Do you like Staten Island as a residence? Is your health very good?" The book attempts to insure that the child will not only know how to contextualize responses, but will also know how to interpret meanings for both their interpersonal and their informative functions (Halliday, 1975). The author suggests that if the adult says "you don't like being shut up so many hours, do you?" the child respond, "I'm so glad spring has come, so I can work in my garden"—not a simple yes or no answer. The author's view is that children should be taught to move the conversation away from superficial social moves to an opportunity for actual information exchange. The child is warned, however: "Very, very few grown people have anything to say worth showing off; but we can any of us say something to please or interest those we talk to" (Power, 1899: np). Whether or not the adult can pick up on this ability of a child is dependent on the adult's ability to handle variety within what we today term "adjacency pairs" in conversation with children (Sacks *et al.*, 1974).

An Egalitarian Ethic and Social Class Distinctions

One of the earliest treatments of politeness formulae proposed that an historical analysis of customs of courtesy would show particular patterns of change correlated with the complexities of distinctions of social rank recognized in a society (Mallery, 1890). Well into the next century, sociologists confirmed the importance of verbal performance as an index of class position, pointing out that either direct or indirect verbal evaluations of speakers and listeners provide "operationally accessible social phenomena by which one can order or measure differen-

tial evaluation" (Barber, 1957:96). The interaction of members of society reflected either equality or differential views of superiority or inferiority.

Discussions of politeness formulae in conversation manuals, grammars, and etiquette books give the reasons and methods for bridging distances between individuals of different social classes. Expression of the degree of overt recognition of social class distinctions necessary for correct usage of politeness formulae changed over the nineteenth century. In the eighteenth century and during approximately the first half of the nineteenth, the most frequent reason cited for the use of politeness formulae was the need for social control. Distrust and suspicion that existed between individuals and classes could not become overt, because of the need to maintain social cohesion. An 1839 manual written for young boys maintained: "Conduct and manner constitute the only dialect which common minds understand, and in them, we must explain those exterior qualities and merits of ours which would otherwise remain unknown to them" (Advice, 1839:112). Overt recognition of different social classes was necessary: "It is impossible to adopt the same notions of propriety in your intercourse with every class of persons and to carry the same system of manners into your relations with people of different ranks . . ." (Advice, 1839:139). Greetings which contained inquiries about health were to be issued as statements, not questions by inferiors to superiors. When visiting in the homes of superiors, inferiors could inquire about the health of superiors by asking questions of servants and then addressing the host: "I am happy, sir, to hear that you are in good health" (Etiquette for Ladies, 1838:56-57). It was critical that speakers know how to judge class differences. Birth, family name, occupation, and wealth were not the criteria; instead, another's superiority was determined on the basis of "age, merits, and the light in which they are regarded by the world" (Etiquette for Little Folks, 1856:32-33).

The fourth decade of the nineteenth century is a time often championed as the era of Jacksonian Democracy, when the

common man began to achieve unprecedented social equality. Jackson has been often charged with setting class against class, and with isolating the aristocracy from the common man. But in the view of recent historians, he cannot so easily be described (cf. Meyers, 1953; Pessen, 1969). Quantitative analysis of social stratification just prior to the Jacksonian era shows that the bonds of family, church, and community—traditional ties to class and determiners of social power—had been altered. People now functioned in a much wider range of institutions as political and occupational ties established new patterns of social organization (Henretta, 1973). Individual initiative for improvement became a mark of morality and character within all professions; each person, regardless of birth, was capable of refinement, so long as he or she applied diligence and sought the proper sources of knowledge⁴ (Mattingly, 1975). It might be expected that if any direct relation were to hold between these societal pressures and customs of courtesy, overt discussions of social class would have been reduced in conversation manuals, grammars, and etiquette books of the period. However, during these decades discussions of social class in these sources continued, advocating self-conscious attention to symbols of self-possession and social assurance.

The effect, if any, of the egalitarian ethic of Jacksonian Democracy on rules for the structure and use of politeness formulae appears to lag several decades behind Jackson's presidency. By the 1860s and 70s, prescriptions for the use of politeness formulae did not openly discuss ways of determining social class nor did they stress the need for different formulae with individuals of different classes. They stressed instead that "gentlemen by

⁴ Wright (1931, 1958) makes a similar point for the middle class in Elizabethan England. Those who wanted to conform to social expectations about language judged written sources more reliable than models of behavior among intimates. See Eble (1976) for a discussion of this phenomenon among women in the early period of American history. Ferguson and Heath (1977) provide analysis of diachronic variation in the structures and contexts of usage of politeness formulae.

birth" were rare, but anyone, by cultivation or "counterfeit," could achieve that status (Duffy 1877:13). Reasons for the use of politeness formulae now stressed not social cohesion, but "mutual dependence" (Bledstein, 1976). The precepts of politeness were now founded on "self-forgetfulness and a respect for the rights and duties of others" (Duffy, 1877:4). Politeness formulae had great equalizing and neutralizing powers in an era when urbanization, immigration of widely diverse ethnic groups, and geographic mobility had greatly increased. Differences existed; they were merely glossed over with politeness (Doyle, 1971). An 1885 writer summarized the role of politeness formulae: "politeness is like an air-cushion; there may be nothing solid in it, but it eases the jolts of this world wonderfully" (Wiggin, 1885:12). The extent of self-conscious knowledge about the changed role of politeness formulae is indicated in an anecdote provided in a children's etiquette book. A mother who had seen her child slight another of admittedly lower class status purposefully slighted her own child in public. Later when the child asked why, the mother responded "I wish you to understand that every shabby, ill-looking creature in the world has just as good a right and cause for attention as you with your style . . . everybody is your equal in right to civility" (Power, 1899:np).

Language in Social Contexts in Historical Data

Historical data can provide sociolinguists a perspective for comparing current interpretations of behavior with those of past societies. Historical methods and interpretations can add depth to on-going analyses of language change. Much current emphasis is given to conversational maxims (cf. Grice, 1975; Gordon and Lakoff, 1971) and ways of describing rules for conversation. These problems are not unlike those of nineteenth-century conversation manuals and etiquette books. In their attempts to portray methods of speaking through a written medium, these sources struggled to describe context, sequencing rules, and ways of determining situated meanings.

The goals of conversation and the imperatives of relevance, information, and brevity are repeatedly elaborated with rationales and rules. Certain rules hold for specific age-groups, mixtures of conversational partners, and spatial settings (for example, the rules for introductions are differently prescribed for church steps and sidewalks). If indeed Grice's (1974) maxims are "social facts," sociolinguists can, with the help of historical data, test the longevity, differing frameworks, and changes in structure and function of these "facts." In addition, historical interpretations may help illuminate the reasons for change over time.

Most sociolinguists have emphasized the interconnections of observable events and the structure of the language they recorded. Speech so conceived has a timelessness which sets it outside history, and places it within a context so circumscribed that it generates what Smith (1962:77) terms the "fallacy of the ethnographic present." Justification of this exclusive attention to the present has rested on the imputed lack of appropriate historical data for analysis of language according to use or users. Social scientists studying current behavior only obliquely admit the past to be one imperative controlling the range of linguistic structures and uses. Present topics, settings, speakers, and situations have been focused on as the springs of social and linguistic behavior. Nevertheless speakers themselves may view their speech habits as explicable, at least in some measure, as a mirror of the past.

If historical investigations are interpreted under a uniformitarian principle (Labov 1972:275), then we can assume that the forces operating today produce changes in language and language use and the interplay of these with beliefs about social class is similar in degree and kind to those which have operated in American history in the past. Combining sociolinguistic questions and theories with historical sources and interpretations of the state of society provides an added dimension to analyses of social structure and discussion of the part played by ideas in social life. In the current sociological examinations of social stratification and

the extent and power of class consciousness in institutions, historical data on language use will prove useful for determining whether or not these institutions altered or perpetuated patterns of social inequality.

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CALL FOR SUBMISSIONS

A number of readers have made inquiries about the possibility of a special issue of *The American Sociologist* on matters relating to academic freedom, both in the United States and abroad, where sociologists and other social scientists are experiencing a variety of pressures. We are contemplating some sort of attention to this problem, either in the form of a special feature or issue, or in conjunction with our continuing but occasional features on social science abroad. We would be most happy to receive either suggestions about such a feature (e.g., specific cases, countries, institutions, etc.) or submitted papers for editorial consideration.

CROSSING THE GULF BETWEEN SOCIOLOGY AND LINGUISTICS*

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Despite considerable activity in the various areas of "sociolinguistics," the disciplinary boundary between sociology and linguistics is quite sharp in contrast with that between anthropology and linguistics. The quality of much research on language and society has suffered as a consequence of the lack of communication between the fields and absence of any exchange of competences. Two areas of research are discussed where interdisciplinary cooperation seems most promising: the social origins of linguistic change in progress, and the analysis of interaction in conversation.

The Location of the Gulf and Some Consequences of It

One of the first contacts between sociology and linguistics was the result of the introduction of the historical linguist Antoine Meillet into Durkheim's Paris circle at the beginning of this century. Meillet (1921) saw that the "laws" of sound change discovered in the 19th century were still only descriptions of what had happened or what might happen; that the fluctuating course of the changes that we actually observe in society could only be accounted for by changes of social structure of the community. This insight was not followed by any investigation of the connection between social structure and language change. It was not until the early 1960s that sociolinguistic research programs were initiated, marked most clearly by the publication of Ferguson & Gumperz's *Linguistic Diversity in South Asia* (1960).

Since then, a considerable volume of writing and research in the area of

sociolinguistics has appeared. Yet the chasm between sociology and linguistics is almost as great as it was in Meillet's time. Only one or two sociologists have learned to interpret the results of linguistic analysis, and there is little evidence that linguists have made use of the concepts and analytical apparatus that are specific to sociology.

The firmness of the disciplinary line between sociology and linguistics comes into even sharper focus when contrasted with the open boundary between anthropology and linguistics. Anthropological linguistics is a flourishing field that advances equally well in the hands of anthropologists and linguists; most of the investigators in this field are hard to classify as one or the other. No matter what index is chosen—programs at meetings, articles in journals, courses taught, or job opportunities—there is clear evidence of intimate relations between anthropology and linguistics, and equally clear evidence of a great distance separating sociology and linguistics.

The fact that anthropological tradition is oriented towards linguistics is obviously connected with the fact that anthropologists have had to learn languages that have never been described before, and it is only natural for them to publish the grammars they have worked out. Many linguists have turned to little known languages for comparisons and reconstructions of language families, and they have needed the techniques of anthropological field work to enter societies where those languages are spoken. On the other hand, linguists have done very little

* This discussion of the relations between sociology and linguistics draws heavily upon my own systematic ignorance of the first-named field, in partial support of the major thesis that the barrier between the two is not easily transcended. I am deeply indebted to a number of people who have tried over the years to disprove the thesis by attempting to introduce me to sociological concepts, notably Herbert Hyman and the editor of this journal. Most of all I am grateful to my wife Teresa for her efforts to correct the most egregious errors in this presentation, and for providing the a fortiori demonstration that a linguist can marry a sociologist and live happily ever after without giving up his fundamental incompetence in the latter field of knowledge.

with the speech of everyday life in their own societies, and so have made little contact with sociologists working in their own countries, who in turn have felt no need for the linguists' help.

The results of this situation have been apparent on the few occasions when able sociologists have turned their attention to language. Schatzman & Strauss (1955) investigated accounts of an Arkansas tornado by people of various class backgrounds. Lacking analytical tools for linguistic analysis, the investigators were confined to impressionistic judgments, such as their opinion that lower class speakers "displayed a relative insensitivity to disparities in perspective" (p. 330), or "think mainly in particularistic or concrete terms" (p. 332). Without objective measures of description or evaluation, the analysts' conclusions are almost inevitably a reflection of their personal preferences and class bias. The only open question for Schatzman and Strauss is "whether the descriptions of perceptions and experiences given by the lower-class respondent are merely inadequate or whether this is the way he truly saw and experienced" (p. 337).

Basil Bernstein (1959) continued this tradition in his efforts to account for educational differentials by class differences in language. Bernstein made considerable efforts to obtain linguistic assistance. Yet the statements that emerged were not informed by linguistic insight, but were dominated by the same bias as the earlier Schatzman and Strauss study. Bernstein's first descriptions of the speech of his working-class subjects were entirely negative: "poor syntactical construction," "simple and repetitive use of conjunctions," "rigid and limited use of adjectives and adverbs" (1959); as late as 1972 he wrote of "the lack of differentiation and the subsequent concretizing of experience which characterizes the restricted code as a whole" (1972:110).

The objective linguistic data provided by Bernstein's group was limited to vocabulary measures such as "number of uncommon adjectives" or unanalyzed grammatical measures like "proportion of passive verbs out of all finite verbs." Such reports have been of little interest to lin-

guists as a whole¹ since they are basically counts of surface phenomena, with no clear connection to the theoretical constructs put forward. The linguistic question is never posed: when a speaker does not choose a passive to express a given meaning, what form does he choose? Bernstein's group did not realize, for example, that the major variable used in basically the same way by all social classes is the alternation of agentless passive ("The closet was broken into"; "The deck was shuffled"), and active sentences with impersonal pronouns ("They broke into the closet;" "You shuffled the deck") (Weiner & Labov, 1977). Instead, they treated these active sentences under a completely separate heading that reflects the normative attitudes of the school teacher: working class speakers are criticized for their improper use of pronouns without specific referents (Bernstein, 1972:110). Bernstein saw a cognitive difference between social classes when in fact he was dealing with a simple difference in fashion: whereas German *Man* and French *on* are freely used in colloquial and formal speech, the English formal *one* is rejected by most speakers in favor of colloquial *they* and *you*.

More carefully controlled studies in Belgium have shown that class differences in the choice of passive vs. active are artifacts of the experimental setting, and can in fact be reversed (Van den Broeck, 1977); other linguistic investigations indicate there is no important difference between social classes in the underlying constraints on the passive (Weiner & Labov, 1977). However, Bernstein's early papers have been widely disseminated among educators who equate formal speech styles with clarity of thought and believe that working class children must therefore be taught to use more passive constructions (Bereiter & Engelmann, 1966).

The other side of the coin is the lack of sociological sophistication in early

¹ A notable exception is M. A. K. Halliday. Bernstein's coding system was an effort to adapt Halliday's system; though Halliday (1975) does not at all endorse the linguistic analyses, he has attempted to induce in other linguists an appreciation of Bernstein's theoretical framework.

sociolinguistic studies of the speech community. Robust patterns of social stratification of language were found in New York City (Labov, 1966), Detroit (Shuy, Wolfram & Riley, 1967), Panama City (Cedergren, 1973), Bahia Blanca, Argentina (Weinberg, 1972), Norwich, England (Trudgill, 1971), Glasgow (Macaulay & Trevelyan, 1973). The major effects of this stratification were strong and emerged with samples as small as 25 speakers (Shuy, Wolfram & Riley, 1967). But the strength of the effect encouraged linguists to neglect statistical analysis, and sometimes to extend the analysis into unreliable subcategories (Labov, 1966: Ch. 9). Multivariate situations were analyzed with univariate techniques; effects of class and ethnicity were examined globally, although the ethnic distributions were highly skewed.²

These early sociolinguistic studies borrowed objective indicators of socioeconomic status from various sociological models (Labov, 1966:216; Shuy, Wolfram & Riley, 1967). The results were gratifying, since indexes combining education, occupation, income, and residence consistently showed higher correlations with stable sociolinguistic variables, and so demonstrated more clearly the highly structured character of the sociolinguistic patterns. (Weinberg, 1972). But not enough attention was given to the way that occupational history, education and life style combined to affect the individual's development and his or her choice of linguistic forms. The regular patterns of stylistic and social stratification led naturally to a structural-functional model, where the various linguistic variables served the function of identifying the speaker's social position and his accom-

modation to the social context. But there was no thorough examination of the alternative conflict models, which have since proven more promising in the explanation of linguistic change.

It is apparent that these sociolinguistic studies of the 1960s would have benefited from more intimate interaction with sociologists who had developed the techniques of survey methodology and multivariate analysis, and conceptual frameworks for analyzing social stratification.

Before considering what can be done to cross the disciplinary barriers between sociology and linguistics, it will be helpful to examine "sociolinguistics," where the same cleavage exists even within the several different approaches to the study of language and society.

The Barriers within Sociolinguistics

The study of language planning and language policy has always been an important component of sociolinguistics. Studies have been carried out by sociologists (Fishman, 1966; Lieberman, 1966), political economists (Kloss, 1966), and linguists (e.g., Ferguson, 1971; Haugen, 1972). The basic data for these studies is the fact that a given speaker X claims to speak a given language Y.

This type of sociolinguistics has been well represented as a growing specialty within sociology; it has often been suggested that it be called the *sociology of language*. It exists side by side with the type of sociolinguistic research that requires a linguistic analysis, yet there is little interaction between the two fields. The linguists who work on the sociology of language use their linguistic knowledge in many ways, but do not engage in linguistic analysis as a part of the research. Fishman is the one sociologist whose research interests cover both areas, but even in his report on the Puerto Rican community the two kinds of research reports are conjoined with very little interaction (Fishman *et al.*, 1968).³

² Examples of such chance fluctuations may be found in the original article developing variable rules in the contraction and deletion of the Black English copula (Labov, 1969). A multivariate analysis using the variable rule program (Cedergren & Sankoff, 1974) corrected some anomalies and found new evidence for the hypothesis of a Creole origin. The original univariate finding that the incoming prestige pronunciation of /r/ in New York City was monotonically stratified by social class was corrected in Labov (1975), where it appeared that the middle working class showed the minimum use of consonantal /r/.

³ Thus, the chapter by Ma and Herasimchuk (1968) in *Bilingualism in the Barrio* uses the techniques of sociolinguistic analysis to describe patterns of spontaneous speech in Spanish and English, yet

At a great remove from the sociology of language is another area where sociologists have played a leading role: conversational analysis.⁴ Ethnomethodologically oriented investigators have moved far ahead of linguists in examining the structured ways that speakers interact: taking turns, delimiting topics, opening and terminating encounters (Sacks, Schegloff & Jefferson, 1974; Sudnow, 1972). At the Ann Arbor Linguistic Institute in 1973, many linguists had an opportunity to work directly with Sacks and Schegloff in this enterprise. Though the linguistic students were profoundly interested in following the analysis, it was my own observation that they had great difficulty using their linguistic skills to contribute to it. Ultimately, an opposition of interests emerged: the sociologists were concerned with the structure of ongoing interaction, and viewed a speech act in terms of what the interactants made of the linguistic event at the time; whereas many linguists, especially those engaged in "discourse analysis," were interested in a grammar of possible speech acts and speech act sequences that might be elicited by introspection outside of any social interaction.

Conversational analysts have asked linguists for help on details of linguistic structure. But the relevant areas of linguistic structure seem to be severely limited: the coding and interpretation of intonation contours, hesitation forms, gestures and other paralinguistic behaviors. Linguists' major area of expertise—the analysis of phonological and grammatical structures—has not been called for. If the highly developed theory of sentence grammar could be directly related to the higher level regularities of verbal interaction, there would be a firm basis for the close cooperation of linguists and sociologists. But the field of discourse

analysis, which would extend sentence grammar to larger units, is still in its infancy. And where linguists have plunged into the study of conversational interaction, the major technical tool that they themselves have needed has been the precise tracing of intonation contours (Labov & Fanshel, 1977).

A third area of sociolinguistics is that set forth by Hymes as "the ethnography of speaking": an examination of the cultural norms that govern the patterned use of language (Hymes, 1968; Bauman & Sherzer, 1974). Here the disciplinary interface is with anthropology, with the favorable setting described in section one. The boundary between anthropology and sociology seems to present no formidable obstacle, since sociologists as well as anthropologists have contributed to the enterprise (Bauman & Sherzer 1974; Ibrahim *et al.*, 1976).

Most of my own work has been in a fourth area, which might be called "sociolinguistic analysis": the examination of the internal structure of the language as it is used in everyday speech. Studies of the social distribution of linguistic variables in time and space fall under this general heading. The first studies of this type were based on individual interviews and survey methodology; since then the range of contact with sociological techniques has broadened considerably. Some of the early limitations in access to sociological techniques were outlined above. More recent developments, which show greater promise of contact with sociological concerns, will be presented below.

The Interface between Society and Language

Current views of the relation between language and society are the result of a series of reactions and counter-reactions to extreme positions. Structural linguists reacted against undisciplined sociological and genetic explanations of linguistic change, and called for autonomous explanations (Kurylowicz, 1964). Martinet (1964) argued that the linguist "may be excused if, in his capacity as a linguist, he declines the invitation to investigate

this analysis is not directly related to questionnaire studies of the use of Spanish in other chapters.

⁴ Here, of course, we are dealing with divisions of some magnitude within sociology. Yet the fact that the approach of Sacks and Schegloff is clearly sociological and distinct from a linguistic approach illustrates that the distance between the two fields is greater than the cleavages in the intent and outlook within sociology.

sociological conditioning." The search for purely internal, formal explanations continues in generative grammar. Sociolinguists have reacted in turn against this extreme position by asserting that linguistic and social structures are co-extensive: that every aspect of linguistic structure may be mapped against some feature of social structure. Yet there is now abundant evidence that the areas of social exploitation of linguistic structure are severely limited.

From a biological perspective, it is clear that language is built on communicative capacities that have a long history in pre-human evolution (for a good overview of this issue, see the variety of approaches in Harnad *et al.*, 1976). In non-human communication systems, the main functions of communicative signals are to identify the social position of the emitter and to accommodate or reject the social claims of the receiver. In human language, the major developments of linguistic structure are associated with the capacity to refer to states of affairs. In actual use, reference identification and accommodation are intimately intermixed, but it is possible to distinguish many aspects of linguistic structure that show no social stratification (group identification) or stylistic stratification (accommodation). Thus, the complex rule of *negative attraction* rules out any occurrence of *any* or *ever* before a negative, so that black gang members and white college professors find equally strange and incomprehensible such constructed violations as "Anyone isn't sitting in that chair, are they?" (Labov 1972b:Ch. 4). It is this most general aspect of language which forms the core of Saussure's notion of *langue*: a Durkheimian social fact that is equally binding on all members of society in both interpretation and production.

On the other hand, the rule of *negative concord*, which produces double negatives, is available to all members of society in interpretation, if not in production. In our investigation of the Philadelphia speech community,⁵ we find in every

neighborhood sharp social and stylistic stratification in the choice of "They didn't do anything" and "They didn't do nothing."

One task, then, is to develop the higher level theory that will state which areas of linguistic structure are open for the social differentiation of speakers and which are not. At present, it seems that most abstract linguistic constraints are quite general throughout the speech community and there is little social significance associated with the forms of such rules. The major social load is on the variable output of the linguistic rules: the words and sounds of the language. Powerful social affect is now associated with the overt markers like *ain't*, suffixes like *-wise*, and the results of ongoing sound change like the raising of short *a* to produce the "nasal" sound in *man* and *bad*. But there is little social affect and little social differentiation associated with structural changes like the wholesale mergers of the word classes of *hock* and *hawk*, *don* and *dawn*, etc., or the ongoing merger of *fool* and *full*, *pool* and *pull* in the Southwest. In recent studies of the passive (Weiner & Labov, 1977), the effects of gender, social class and ethnicity were negligible in determining the use of active or passive.

Thus, it appears that the outputs of linguistic structures, rather than the abstract rules that produce them, are the carriers of social significance. This does not mean that social factors do not play a major role in the evolution of language. The effect of social constraints on words and sounds may inhibit, promote or alter the form of output rules; and by the addition of later constraints, the same rule may over time assume a higher position in the hierarchy of abstract ordered rules. There seems to be no doubt that social factors were important in the development of the Great Vowel Shift in early modern English (Wyld, 1936). But those social factors are now encapsulated in a very abstract relation in English phonology (Chomsky & Halle, 1968) which may not easily be recognized by speakers today; any alternations that remain have no social affect whatsoever. Thus, the alternation between *Cooper* and *Cowper*, or two pronunciations of *Houston*, are quite discon-

⁵ The research referred to here is part of the project on linguistic change and variation at the University of Pennsylvania, supported by the National Science Foundation under grant BNS76-80910.

nected from the social significance they may have had when they originated three centuries ago.

Even when powerful social factors are at work, it must not be assumed that the linguistic variable is available for all social functions at all times. This can be seen most clearly by tracing the typical trajectory of changes in the sound system of a language, stages in the almost continuous rotation of English vowels that is responsible for the great differences between sound and spelling in our language. Sound change is in fact the basic mechanism responsible for the diversification of dialects to create languages as distant as Hindi, Russian, English and Welsh from what was once a single mutually intelligible form of communication. From their earliest attempts to understand this extraordinary phenomenon, linguists have debated the question: what is the role of social factors in the propagation of sound change?

Our present understanding of this issue shows a variety of relations between language and social factors as a sound change progresses. In its earliest stages, a sound change is associated with a given social group, as an indicator showing social differentiation without any shift in stylistic context. Such indicators are so far from conscious awareness that dialectologists and phoneticians may discover them only after spending several years in the community. At a later stage, stylistic shifting may develop, and the variable may show more advanced forms in one social setting than another, or even be corrected to a base form in formal contexts. It thus functions as a marker of both social position and contextual style, and produces the double stratification that appears in sociolinguistic studies (Labov, 1966; Trudgill, 1971). Finally, the feature may arise to full social consciousness, and be labeled and discussed as a linguistic *stereotype* related more or less closely to the forms that people actually use.

In the light of this gradual development of social awareness, it follows that the later stages of a sound change are relatively easy to chart, while the early stages are far more difficult to isolate, and it is not an easy matter to determine the causes

of the original linguistic differentiation of the social groups involved.

Two Areas for Joint Research

a. *The social causes of sound change.* Given the rich variety of sociolinguistic variables, it seems clear that linguistic indicators have a great deal to offer sociologists as means of tracing social processes (Labov, 1968). It is not likely that many sociologists will have the training in linguistic analysis that would give them ready access to these variables. It seems more likely that linguists and sociologists will work together to explicate the social forces that have produced the sociolinguistic patterns already located.

One problem which calls out for joint analysis is the explanation of the current findings on the social origins and propagation of sound changes. Nineteenth-century linguists advanced two types of explanations for the sound change, the major factor in linguistic change and evolution: (a) the principle of least effort, and (b) the principle of imitation. In either case, it would follow that linguistic changes would originate at one or the other extreme of the social spectrum. Those who are in the lowest social position would be most affected by the principle of least effort, would have the least contact with prestige forms, the least opportunity to imitate them, and the most tendency to drift away from them. Those in the highest socio-economic position would be most imitated, would have the greatest command of prestige forms, and would be least likely to borrow from others.

In the various sociolinguistic studies of speech communities mentioned above, it gradually appeared that sound changes do not originate in either of these social groups, but rather in centrally located classes in the socio-economic hierarchy: the upper working class or lower middle class (Labov, 1972a:Ch. 9; Labov, forthcoming). These results were based on impressionistic phonetics that have long been known to be limited in reliability (Ladefoged, 1967). The Philadelphia study is designed to achieve more precise statements of these developments through in-

strumental measurements of the vowel systems of several hundred speakers, across a wide range of neighborhoods, social classes, ethnic groups and ages. The timbre of a vowel—its characteristic “ee,” “o” or “a” quality—can be expressed in terms of the frequencies of two concentrations of energy in its spectrum (the “vowel formants”), and changes in the mean values of these frequencies correspond to changes in the vowel quality. For example, one of the most recent and vigorous changes in Philadelphia is the change in the vowel of *now*, *down*, *out*, etc. For older speakers, this diphthong begins with [æ], the vowel of *mat*. For younger speakers, the vowel shifts so that it begins with [e], the vowel of *mate* (though the sound is always different from both *mat* and *mate* since it has an additional off-glide).

This change in the vowel of *now*, *down*, etc., can be measured as a change in frequency of the vowel formants within fairly narrow limits.⁶ These measurements can then be entered into a multiple regression program which yields an equation for each sound change with coefficients representing the contributions of gender, age and social class to the progress of the change. The quantitative results may be expressed in units that represent percent of the total possible change within limits of the acoustic space available—if the initial vowel position were to shift from the lowest (“a”) to the highest possible position (“ee”). The range of variation found in the community today is about half of this value. The contribution of each year of age is .63 in these units, so that the mean value-for speakers age 20 would be 25.2 units further along in the change than speakers age 60. As in most of the sound changes studied, women are considerably ahead of men; membership in the female gender contributes 8.7 units. It is also clear that the sound change has originated in the upper working class. The effect of

membership in each class (considered as a nominal variable) may be represented as lower working class 4.4; upper working class 13.2; lower middle class 6.7; middle middle class 2.8; upper middle class 0.

These quantitative results reinforce the general finding that linguistic change originates in social groups centrally located in the class hierarchy. The job now is to explain that finding, and here the joint insights of linguists and sociologists are needed.

In some of the earliest work on the social origins of sound change, the concept of “local identity” appeared quite prominently. On the island of Martha’s Vineyard, a complex distribution of the centralization of the diphthong /ay/ in *right*, *nice*, etc., was correlated most closely with positive orientation towards the island, and the entry of new social groups into the rights and privileges of island life (Labov, 1963). But this concept of local identity was an impressionistic one, characteristic of a judgment sample, and the local rights and privileges were only roughly documented. The study of a similar problem in a large city poses more difficult problems.

It seems clear enough that the highest and lowest status groups are not involved in local values or local rights and privileges. The lowest class, largely black, is excluded from those privileges and is oriented towards a nationally uniform black vernacular culture. The white upper middle class rejects local values and adopts national reference groups with high prestige. The centrally located groups appear to be the most local in character.

It is not clear how the pressure to preserve local rights and privileges can result in a relatively uniform metropolitan dialect. In both New York and Philadelphia, the dialect similarities among neighborhoods and among white ethnic groups are much greater than the differences. Yet the local prestige that has been observed in Philadelphia is not based on membership in a metropolitan community, but on status in a local neighborhood often much smaller than a census tract. It is easy to see the pressures on ethnic neighborhoods to preserve their identity and resist invasions, and it is easy to conceive of sound

⁶ There are a number of technical problems that must be solved if these measurements for various speakers are to be superimposed in a single grid, since the different vocal tract lengths of men, women and children result in different acoustic dimensions for their vowel systems (Hindle, 1977; Lennig and Hindle, 1977).

change as a response to that pressure which symbolically re-asserts local identity. But it remains to be explained how Irish neighborhoods in North Philadelphia advance in lock-step with Italian neighborhoods in South Philadelphia. How do local features become generalized to all members of a social class?

We are inevitably drawn to the study of communication networks. The concept of open and closed networks was first introduced into sociolinguistics by Gumperz (1964), in his work on the use of local and national dialects in Hemnes. In South Harlem, we used sociometric techniques to delineate central and peripheral members of adolescent peer groups, and related these to quantitative indices of distance from the Black English grammar (Labov, 1973). In Philadelphia, we have begun to develop communication indices that would help to explain the spread of linguistic influence. At this point, however, it would seem that a more systematic attack on the problem is called for, which would match in effectiveness the elegant and convincing methods of Katz and Lazarsfeld in tracing personal influence (1955).

The parallel problem in the spread of linguistic influence appears in the examination of linguistic evidence for reference groups across racial lines. In racially mixed North Philadelphia schools, there is evidence that Puerto Rican children have moved towards the Black English vernacular as a prestige form in casual style, a tendency strongest among boys; and conversely, have shifted towards the non-standard white Philadelphia vernacular in formal speech, a tendency strongest among girls (Poplack, 1977). The general pattern is characteristic of Puerto Rican peer networks even in a classroom where there is only one black child. It is evident that the future language and culture of the inner city will be further influenced by the large black population; how is this influence disseminated and how extensive will it be?

Another challenge to an understanding of communication patterns is an explanation of larger distributions of linguistic change. Callary (1975) studied the pronunciation of short *a* words (*cat*, *bat*,

etc.) among freshmen girls from various communities in northern Illinois. He found a very regular pattern: the larger the town, the more advanced the sound change. There is some evidence that the same configuration is found in lower Michigan and upper New York state. Location of the communication patterns that are responsible for this effect will advance our understanding of the mechanisms of both social and linguistic change.

b. *The social basis of coherence in conversation.* In the study of conversational interaction, neither linguists nor sociologists confront the problem of mastering a large body of technical information. The study of discourse is less developed than the study of grammar and phonology, and linguists and sociologists are on an equal footing in their ability to deal with conversation. But given the radically different orientations outlined above, how are they to work together?

First, it is worth examining some obvious limitations in the formal linguistic approach to discourse analysis. Conversational postulates that have been proposed (Gordon & Lakoff, 1971; Grice, 1975) are based on imagined conversations which have only a limited resemblance to the conversations recorded from social interaction. The discourse rules deal only with the simplest kinds of indirection: for example, declarative statements of need as ways of making requests for action. Moreover, the rules are limited to the distribution of referential information about needs and abilities. In actual conversations, the basic coherence of the sequences is more often based on social rights and obligations: their assertion, challenges to and retreats from those assertions, supports and defenses (Labov & Fanshel 1977:Ch. 3).

In general, linguists have developed their intuitions about grammatical structures, but do not have ready access to their native knowledge of social relations. The analysis of narrative and the understanding of the connectivity of events depends on an ability to isolate common assumptions about social life. I find an extraordinary resistance among linguists to recognize the understanding that they have as members of our society. For

example, a Philadelphia adolescent told a story in a recorded group session about a night on the Jersey shore, when he and a group of his friends met a girl calling out for help. Her boy friend, a lifeguard, had passed out in a lifeboat. The group charged down, picked up the boat, and carried it down the beach shouting and singing, ignoring the girl who was running along beside them—until the cops arrived and chased them all over town. The narrator doesn't explain why they did what they did; he assumes our general understanding of a lifeguard as a social type (Klapp, 1962). Though linguistic students have no difficulty in calling up grammatical intuitions, it is not easy to elicit from them the native knowledge that they obviously use in listening to this story: that the stereotyped lifeguard is obnoxious to other youth because he (1) sits around showing off his body and his tan, (2) bosses everyone else around, and (3) attracts girls who sit around and admire him.⁷ Those trained in anthropology or sociology have much less difficulty in bringing their social understandings to focus on a text.

On the other hand, linguists can contribute to this enterprise an awareness of abstract and hierarchical structure that underlies conversational interaction, drawing on their familiarity with parallel operations in syntax and phonology. Other analysts tend to rely upon their intuitions, going directly to what a text "really means," without observing the distance from the surface utterances.

I have just completed ten years of close cooperation with a sociologist in the enterprise of analyzing discourse between a patient and a therapist (Labov & Fanshel, 1977). At first, I expected to contribute measures of style shifting, using the kinds of indexes discussed above. It soon became evident that the major contribu-

tion of linguistics to the study of conversation is the ability to unravel in a single utterance the multiple layering of many speech acts. But without the guiding impetus of sociological insight, the analysis will remain at a superficial level of reference and request, and trivial answers will be given to the fundamental sociolinguistic question, "Why does anyone say anything?"

For example, in our study of therapeutic discourse, a daughter calls up her mother, who is babysitting for a married sister, and asks "Well, when do you plan t'come home?" It is plainly a request for action. But the linguist also focuses on the fact that the utterance is most immediately a request for information about a certain time, and will be interested in working out the unstated rules by which a native speaker can recognize such a request for information as a request for action. These abstract and hierarchical relations of surface structure are valuable guides to an understanding of the social interaction. They reveal the depth of indirection and mitigation of requests, and show the range of multiple connections that may be used in responses.

We would have only a poor understanding of the utterance just quoted, "Well, when do you plan t'come home?" if we remained at the level of requests for action. It is important to see that the request to come home is itself a means to another speech act, a challenge by the daughter to the competence of the mother in carrying out her primary obligations to the household.

My first contact with Harvey Sacks was in a small group where we discussed this data. Sacks was quick to recognize the structural basis for the challenge. In our society, Sacks pointed out, it is normally the mother who asks the daughter to come home, not the other way around. This reversal of roles underlies the challenge to the mother's competence. Secondly, Sacks pointed out that the word "Well" as a discourse opening normally refers to some unstated but relevant fact which both parties recognize: in this case, that the conditions for the request to come home have been in existence for several days; it is therefore an overdue request.

⁷ Cf. the popular song, "I Wanna Be A Life Guard": "I wanna be a life guard/Just a public servin' life preserving' man/I wanna be a life guard/With a million dollar coat of tan/I'll sit up high on top of a wave/And keep my eye on every cutie while on duty/So if you want a life guard/You can bet your life that I'm your man." Words by Bob Rothberg, music by Sammy Timberg. Copyright © 1936 by Famous Music Corporation. Copyright renewed 1962 by Famous Music Corporation.

We eventually formulated a general Rule for Overdue Obligations which is responsible for the nature of the challenge involved here: "If A asserts that B has not performed obligations in a role R, then A is heard as challenging B's competence in R" (Labov & Fanshel, 1977:96).

The study of discourse is only gradually approaching an interconnected body of general principles, and is a long way from the kinds of quantification appropriate for grammatical analysis. But in this area it seems to me that progress will be made through intimate cooperation of linguists and sociologists in constructing a deeper and more realistic analysis of conversation.

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THE USE OF PERSONAL DOCUMENTS IN HISTORICAL SOCIOLOGY*

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Methodological literature concerning the use of historical personal documents in sociological research has been limited and diffuse. Our aim is to integrate the ideas in these discussions in an attempt to revive the use of personal documents. Using ideas of historians and sociologists, we examine both a data source and a method, and describe their relationship in producing a valid and reliable accounting of an event. We examine this type of data in terms of four criteria: representativeness, adequacy, and reliability of the data, and validity of the interpretations. While definitive guidelines for the use of personal documents are impossible to recommend, we suggest ways of recognizing and dealing with many difficulties throughout the analysis.

INTRODUCTION

American sociology has increasingly emphasized quantitative research, and has tended to neglect more humanistic, qualitative and historical strategies. The methodological orientations and scholarly approaches that underlie the still relevant classics of sociology have been little used in recent work. Some early (Thomas and Znaniecki, 1918) and recent (Rock, 1976) sociologists have discussed the use of historical personal documents in sociological research, but contributions to the literature have been few, and have been written from such different perspectives that their usefulness is limited. An integration of these studies is needed to revive and reactivate the use of personal documents in sociological research.

Borrowing from historians as well as sociologists, we describe both a data source and a method, and examine their contributions to the valid and reliable accounting of social events. We systematically explore the nature of historical personal documents, discuss their benefits and limitations, and provide certain guidelines for scholars interested in working with such materials.

To study certain problems in the social sciences the use of personal documents is

essential. Any sociological exploration of the past which assesses the perspectives, attitudes, or motivations of participants in an experience requires the examination of these data. Furthermore, personal documents may constitute the only extant data regarding social phenomena no longer in existence, such as organizations or movements. Consequently, practical guidelines for working with these materials would be valuable to many sociologists.

Definitions of Concepts

Gottschalk (1947:8) defines the historical method in social research as the process of critically examining the records and survivals of the past. Three steps are associated with this method: (1) collection of probable sources of information; (2) examination of these sources for authenticity, either in whole or in part (see Barzun and Graff, 1977); and (3) analysis of the data collected through this process.

The reconstruction of the past from the data derived in the above process is called historiography. Historical sociology is an approach to historical data, a style of historiography, that seeks to explain and understand the past in terms of sociological models and theories. Sociological concepts and principles may be used to describe and analyze actual historical situations on a higher level of abstraction and generalization. Alternatively, historical data may be used to illustrate and test the validity of sociological concepts, princi-

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ples and theories (Cahnman and Boskoff, 1964:8; Hughes, 1960; Lipset, 1968; Social Science Research Council, 1954).

Several important contemporary works illustrate the synthesis of history and sociology advanced in this paper, using personal documents to examine various general sociological principles. They include, among others, Erikson's (1966) application of Durkheimian theory to the history of deviance in the Massachusetts Bay Colony; O'Dea's (1957) interpretation of Mormon history; the work on social change in the Industrial Revolution by Smelser (1959, 1967); and Marsh's (1961) application of Pareto to the Mandarins of China.

According to Angell (1947:177), a personal document is defined as material which reveals a participant's view of experiences in which he or she has been involved. The individual may reflect upon his or her own activities or attitudes, or may attempt to describe groups or issues with which s/he is involved. The document is generally written by the actual person, but careful interviews which omit the interpretations and biases of the interviewer (for example, oral history recordings and transcripts) may also be considered personal documents. Gottschalk (1947:15-27) provides a valuable catalog of such documents, some of which are more distinctly personal than others. They include such items as stenographic records, business papers, memoranda, diaries and personal letters, newspaper reports, autobiographies, government documents such as consular reports or testimony in public hearings, newspaper reports and fiction. Additional sources might include such materials as photographs, films and certain handbills, flyers, manifestos, etc.

The Uses of Personal Documents

Personal documents provide a unique subjective orientation to the experiences they describe. In the tradition of Thomas and Znaniecki's (1918) celebrated "methodological note," we maintain that the analysis of subjective meaning is critical in the exploration of human behavior.

Angell (1947:180-186) identifies six

ways in which personal documents (contemporary or historical)¹ may be used: (1) to secure conceptual "hunches"; (2) to suggest new hypotheses within the context of an established framework; (3) to provide data for impressionistic accounts; (4) to verify formal hypotheses; (5) to validate a conceptual framework; (6) to illustrate concepts and establish principles that are used in scientific analyses. Documents used in this last way clarify scientific abstractions by tying them to concrete events.

Allport (1942:53) uses the terms *nomothetic* and *idiographic* to distinguish between two uses of science: to abstract from particular cases in the creation of general principles; and to use a theoretical model to illuminate a particular case. We are primarily concerned here with promoting the idiographic mode of research (see also Cahnman, 1976a).

METHODOLOGICAL CONSIDERATIONS

In his well known critique of *The Polish Peasant*, Herbert Blumer (1939) identifies four criteria for evaluating any data source in the social sciences: representativeness of data; adequacy of data; reliability of data; and validity of the interpretation. We will examine each of these four criteria.

Representativeness of Data

It is difficult in historical research to evaluate how adequately the body of available materials represents the universe of activities, attitudes and feelings which characterized a particular historical

¹ We should emphasize, at this point, that the guidelines established in this paper are appropriate to personal documents involving past rather than contemporary events. In the analysis of the present an entirely different set of assumptions and procedures would be operative. For example, since the representativeness of the data can be assessed in contemporary, but not historical documents, inductive generalizations are more credible. Similarly, specific lines of questioning may be pursued with living persons and independent cross-checking with co-participants may assist in establishing veracity. The validity of interpretation, particularly with regard to such matters as motivation or intention, consequently, may be improved.

instance. Historically oriented sociologists face many of the same problems that hinder other social scientists who work with preassembled data. The major constraint in using primary data sources is that data collection and transmission procedures are beyond the control of the researcher. Thus, the materials available are not a matter of choice; rather they are artifacts of what was recorded and what has survived over time. The historical researcher must account for biases and gaps in the data that have survived, and must explain the extent to which the conclusions are affected by the deficiencies. At times, gaps may be filled in through imaginative reconstruction if the author has a full command of the available data and its interconnectedness.

Although it presents a special set of challenges, the collection of personal documents and other historical data involves a process similar to other types of bibliographic research. Excellent guides to historical research by Barzun and Graff (1977), Shafer (1974) and Gottschalk *et al.* (1947), provide the newcomer with a starting point. The first step in historical research is to identify and locate materials, many of which are available in state, local or university collections. The persons, area, times and functions involved in the matter under investigation must be carefully delimited. Pertinent dates and locations must be surveyed, and the movements of individuals traced throughout their lives to get clues about potential sources. Furthermore, familiarity with relatives or acquaintances with whom individuals have established contacts may lead the researcher to letters that have been deposited in public archives.

However, needed materials frequently exist only in family archives or special libraries. There is a wealth of materials available in one published form or another. Local and state historical societies have traditionally undertaken the publication of documents and ephemera relevant to local interests. In addition, modern printing techniques have made mass duplication of documents practical, and a number of publishers such as Arno Press, Porcupine Press, AMS Press, Augustus M. Kelly and Greenwood Press have

begun reproducing all types of documents including collections of letters, memoirs, original ideological sources, contemporaneous and secondary sources. Newspaper reprints and microfilm reproductions are also available.

Unfortunately there is no reliable test available today that can tell the researcher whether or not the surviving data are a representative sample of experiences. Thus, the researcher should remain skeptical and assume that enormous biases pervade the historical remnants. Such biases are the consequences of *whose* materials are preserved and *what* experiences are recorded.

In some instances, an elitist bias pervades the preservation of materials. Often this is due to the highly skewed distribution of literacy in previous eras.

Several factors affect the matter of *what* gets saved. Rock (1976:356-357), operating from the perspective of ethnomethodology, notes that there are broad areas of both interior and exterior experience that inevitably escape documentation. Because they do not necessarily parallel verbal expression, emotions exemplify experiences that may not become accurately recorded. Furthermore, immediate experience is only occasionally put into writing, and always retrospectively (Rock, 1976). A wide variety of shameful and/or intimate experiences escapes documentation for no other reason than to guard the respectability of the author.

The researcher must also be aware that experience is very selectively recorded. Historical documents may be ranked in order of how consciously the author was speaking to a historical record. Official histories or items written for recruitment or propaganda purposes are found on one end of the continuum, while diaries and notes of all kinds, which initially were not written for the "record," are found on the other end (Allport, 1942:95). The researcher should immediately question the representativeness of the materials written by an author who was fully aware that future evaluation of the experience would be based on the document. In contrast, items written without regard to their possible use as interpretive materials should offer a wider representation of ex-

periences and present fewer problems in this regard.²

Despite these limitations, historical personal documents offer many advantages. Most importantly, these materials allow the sociologist to observe reality from the perspective of the subject without letting his or her own social scientific perspective shape the reality which is conveyed. A person speaks to the historical record in his or her own idiom. Impressions are not shaped and channeled by response categories on a questionnaire or an interview schedule. Of course, this eliminates the possibility of pursuing a line of questioning or probing the depths of a subject's level of meaning (Rock, 1976), but it also eliminates the influence of the researcher upon the perspective of the respondent.

Adequacy of Data

Since there is no possibility of interaction with the historical subject, nor any possibility of pursuing a larger sample, the issue of generating an adequate amount of data is out of the sociologist's control. There is no way to evaluate the depth or comprehensiveness of the material that has been conveyed. However, there are several means by which the historical sociologist can make educated guesses about the issue of adequacy. First, s/he should critically examine the scope of material available to ascertain whether it reflects the full range of responses to the event, in other words, whether recollections are balanced with regard to coverage. But since a fully adequate sampling of materials is hardly ever available (Cahnman and Boskoff, 1964:4), the researcher should evaluate the gaps and suggest areas that need further examination when additional sources of data are found.

Research into communitarian societies, such as Owenite New Harmony (Mariampolski, forthcoming), may exemplify this process. Because these groups have often promoted styles of life highly at variance with prevailing social norms, both inside

and outside observers have dwelled upon the most experimental or salacious aspects of the groups. At New Harmony, the particular—and most reported—focus of the venture was its educational reformism and religious iconoclasm. It is difficult to find theoretically important material on such aspects of life as work organization and family relations within the available documentary evidence.

Another means of guessing at the adequacy of the data involves making assumptions about broad commonalities within human experience. Our own experience may be a guide to other times and places because "the ranges of human experience are so constrained that they do not radically separate men of different eras and situations" (Rock, 1976:365). The researcher's own actions in social worlds should provide the basis for a sense of empathy with historical actors that will help the researcher to evaluate the experiences of others. Of course, a person's motivations and modes of thought vary with different historical contexts or with different phases of development. These two considerations must be balanced against each other.

Reliability of Data

The reliability of data essentially involves (1) the credibility of the data source and (2) the accuracy of transmission of the data recorder (Aveni, 1972). It is impossible to eliminate distortions in the historical record and difficult to assess their extent and consequences. However, guidelines may be established for evaluating the degree of distortion and estimating the reliability of the historical record.

Historians often criticize sociologists for being insufficiently critical and skeptical about materials which they put through analysis (see Erikson, 1971). In fact, sociologists frequently do base elaborate arguments on sketchy and fragmentary data or upon only secondary data sources. A healthy skepticism regarding historical materials is something sociologists should develop.

Several factors will inevitably conspire to produce distortions in data. The political or ideological orientations of the data

² See Gottschalk (1947) for an arrangement of the seven groups of personal documents in order of their general reliability, and a discussion of their representativeness.

source or recorder may bias his or her perspective, and thus leave open the possibility of mistaken inferences (Aveni, 1972:8). Distortion may also follow from the perspective generated by the particular organizational role of the data source.

A recent example of this problem involves a challenge to the historicity of Lipset and Riesman's (1975) assessment of the McCarthy Era at Harvard. A participant in the events (Diamond, 1977) provides evidence of his own experiences which tends to contradict the depiction of the Harvard administration as relatively benign and protective of its politically dissident faculty. The actions of university leaders in his own admittedly "deviant case" would suggest, Diamond asserts, that an alternative interpretation may be warranted.

Distortion may also arise in the transmission of information. Rock (1976:357) points out the selectivity that operates in the recording and documenting of experience. Material is frequently generated in a manner that saves the respectability and propriety of the data source. Furthermore, there are historical jokes or disguises meant to be recognized by individuals with special insight or familiarity with the conditions being described. A writer who is aware of who will see the documents often attempts to protect and disguise points of criticism.

The descendants of historical personages are also concerned with matters of propriety and respectability. Thus, they must also be treated as potential sources of distortion. Changing codes of morality and/or the upward mobility achieved by offspring may lead them to hide and distort elements of records. The respectability of subsequent generations may be influenced by the salaciousness of an ancestor noted for deviation and experimentation in life style. As Erikson (1971:71) points out,

... the one quality all these documents have in common is their survival; and even though a researcher has complete faith in the authenticity and reliability of the documents, he must wonder by what law or accident they came to be preserved. They are not the random remains of a dead age, like the debris found at an archeological site. Every genera-

tion of men that has lived meantime has served for a period as custodian of those records, and thus the surviving library of materials is in many ways a record of all the intervening years as well.

The history of utopian experiments provides an interesting example. Descendants of the Oneida Community, embarrassed over the details of the group's practice of communal sexuality, burned nearly a truckload of diaries and letters containing frank and intimate disclosures of involvements. Consequently, much information about personal aspects of the lifestyle has been lost (Kephart, 1976:84-85).

Of course, some sets of documents may be less vulnerable to this problem than others. For example, materials that are sealed for substantial periods of time, particularly government documents, are not threatened by embarrassed descendants. Nevertheless, questions of preservation are as important as those of recording. In either case the researcher should be wary of a purposefully distorted legacy.

Although the issue of reliability may never adequately be laid to rest, there are guidelines to help the researcher estimate the amount of distortion and avoid pitfalls. These principles guide the sociologist in maintaining sufficient skepticism regarding his or her data (see Erikson, 1971:70-71).

Perhaps the best statement on the evaluation of evidence for reliability is by Gottschalk (1947:30). He argues that although the criteria for evaluating evidence are not as rigid as those in a court of law, four tests should help establish how dependable any body of personal documents may be:

1. Was the ultimate source of the detail (the primary witness) *able* to tell the truth?
2. Was the primary witness *willing* to tell the truth?
3. Is the primary witness *accurately reported* with regard to the detail under examination?
4. Is there any *external corroboration* of the detail under examination?

The ability to tell the truth involves a number of conditions, such as the witness's nearness to the event, both geographically and chronologically, the competence of

the witness considering his or her degree of expertness, state of mental and physical health, age, education, memory, and narrative skill, to name a few. Selective inattention and a myopic view of events similarly impede reliability.

A major factor jeopardizing the ability to tell the truth is the egocentrism of the author. According to Gottschalk (1947:40), "It is to be expected that even a modest observer will tell what he himself did as if those details were the most important things that were said and done."

Concerning willingness to tell the truth, Gottschalk (1947:40-42) summarizes several kinds of untruthfulness that researchers should be aware of. A blatant or subtle bias may result from the author's religious, political, economic, racial, regional, family and other personal ties. The desire to please or displease a particular audience may color or even erase certain facts of a situation. Expressions made in a coercive context, such as a prison, may also be distortions of fact. An author given to bombastic phraseology may sacrifice the truth in favor of literary style. Etiquette in letters and oral conversation and conventions and formalities in treaties and public documents require expressions of esteem that are often false or empty, distorting an individual's role.

Finally, expectations or anticipations of the witness may result in untruthfulness. Witnesses with preconceived notions often find what they are looking for, since they fail to observe data contrary to their expectations. Those who have worked with historical documents relating to religious or utopian movements are familiar with this problem. Millenarian expectations often impede the critical senses of followers and tend to result in wildly fanciful reports.

- Unwillingness to tell the truth, whether intentional or unintentional, results in misstatements of facts more often than omissions of fact (Gottschalk, 1947). Errors of both omission and commission are found in documents in which the authors were both unable to tell the truth and unwilling to tell the truth.

Gottschalk (1947:43) also delineates conditions that are favorable to truthful-

ness. A witness is likely to be unbiased and truthful when: the truth of a statement is a matter of indifference; the facts are a matter of common knowledge; the statements are both so incidental and so probable that error or falsehood seems unlikely; or the statements are contrary to the witness's expectations or anticipations. It is important to note that a skillful liar is also aware of conditions favorable to reliability and may easily deceive the unwary investigator.

Gottschalk (1947:44) argues that whenever possible, researchers should use primary, i.e. eyewitness, testimony. When such sources are not available, the closest secondary source available should be used. Frequently, testimonies of secondary sources have great validity, especially when they attempt to resolve contradictory accounts. However, secondary sources should be evaluated scrupulously and accepted only cautiously. The researcher should carefully question the secondary recorder's biases and source materials.

While a primary particular which has been extracted from a document by the process described so far is presumably trustworthy, it is not yet regarded as reliable (Gottschalk, 1947:45). The general rule is to accept as historical only those particulars which rest upon the independent testimony of two or more witnesses. Independence as witnesses must be established; otherwise agreement may be confirmation of a lie, or of a mistake, rather than corroboration of a fact.

When it is impossible to locate two independent documents recording the same facts, the investigator must look for other kinds of corroboration, such as the absence of contradiction in other contemporary sources, the general reliability of the document, reputation of the author for veracity, lack of self-contradiction within the document, and the way the document conforms to, coincides with, or fits into the otherwise known facts. Nevertheless, various contradictory statements may have equal reliability if the interpreter is aware of and is able to appreciate the different points of view from which the statements are made.

Validity of Interpretation

Because of data sampling problems and the qualitative nature of historical personal documents, it is often difficult to apply statistical methods that permit interpretive judgments based on the language and logic of probability. Thus, the historical sociologist is frequently forced to find alternative ways of explaining relationships between the variables under examination. A careful argument supported by the cautious exposition of evidence which has gone through a skeptical analysis is the historical sociologist's substitute for statistical inference. Three processes are critical in the development of a sound reasoned argument: (1) a sensitive commitment to a theoretical perspective; (2) thorough familiarity with the documentary material and a sense of empathy for its contents; and (3) the ability to imaginatively, even artistically, interrelate evidence and theory.

Historical sociology fosters an understanding of history in terms of systematic models of social behavior. The researcher may bring a model of social processes to the data that has been collected and evaluate the "fit" between historical evidence and the expectations generated by the theory (see Smelser, 1967). Or s/he may allow theory to develop from relations inherent in the historical example (see Cahnman and Boskoff, 1964). In either case, the theoretical principles should be flexible rather than reified. Cahnman (1976:116) argues that theory should be "a guide, not a master. . . ." It should be used to clarify and expand historical visions rather than to lock history into a mold of iron determinism. Balance should be achieved between the nomothetic and idiographic possibilities of historical sociology. The researcher must not lose sight of the unique case in the search for patterns.

A purely inductive approach to historical data is impossible because of problems with representativeness, adequacy and reliability of the data. The researcher must ultimately develop a sense of balance and proportion concerning the data and an intense capability for judgment. His or her own biases and inclinations will inevitably

interact with the visions that emerge from the documents. This partisanship should not be shunned; on the contrary, the researcher will discover natural inclinations leading toward the support of one or more features of the data at the expense of others. A sense of history *in vivo* emerges from the fugitive documents. Just how this sense of empathy and insight is achieved is difficult to say in terms of a specific training program. Cahnman (1976b:835-836) refers to a "trained imagination" which is able to pick what is "essential" through the "free exercise of craftsmanship." Practice in working with historical documents will help to develop a "feel" for history, just as practice will help to develop an "ear" for music. The researcher should become completely familiar with all of the remains of the historical event, and even visit the site if possible.

Our objective in encouraging a sense of empathy is to narrow the gap between researcher and subject. Empathy, rather than causing distortion, may actually undermine it by introducing sense and balance to historical images.

The reconstruction of history requires an imaginative reflexivity between evidence and interpretation. The process is inevitably ambiguous. Any interpretation, whether of participant observation data (see Becker, 1958), past statistical records, or psychiatric interviews, is necessarily tentative. In every case the researcher must develop hypotheses or models and test the evidence as "objectively" as possible, noting deviant cases as well as those which support the theoretical frame which informs the research. "Proof" in working with materials which reflect human thoughts and values (as contrasted to physical attributes) frequently lies in the compelling nature of an author's language and in the elegance and parsimony of an interpretive perspective. Indeed, "proof" is never possible in historical sociology; we can only present our best argument, knowing that it may be overturned by more complete data or a more encompassing theoretical perspective. A recent controversy over the relationship between clothing styles and the roles of Victorian women exemplifies both the tenuousness and the potential of the

interpretation process. Roberts (1977) argues that women's clothing of the period promoted inactivity and submissiveness, while Kunzle (1977), producing different evidence, maintains that lacing represented an assertion of female independence.

DEVELOPING A HISTORICAL ARGUMENT

The process of developing a historical argument is known as historical synthesis. The following discussion is based on Gottschalk's (1947:48–62) analysis of this process. For the historical sociologist, many of these issues will be affected by the peculiarities of the perspective that he or she seeks to apply to the data.

In addressing a historical thesis, the sociologist must choose from among the multifarious bits of evidence that have accumulated. This is the problem of establishing *relevance*. A rule of selection will clearly be linked to the specific theoretical notions being demonstrated. Similarly, the *arrangement* or order in which the data are presented is problematic. The specific arrangement selected should have relevance for the problem under investigation. (See Gottschalk, 1947:50–51 for elaboration on this point.)

The problem of *emphasis* is closely related to problems of selection and arrangement. Obviously not every detail that has been accumulated is of equal importance. It is frequently necessary, furthermore, to negotiate between two somewhat different versions of the same event. Consequently, the researcher must determine a standard by which a particular view of events or a specific complex of bits of information will be highlighted. Exhaustive lists of details are as useless, and frequently as distorted, as biased or manipulative accounts.

Assigning *cause* to historical events is an enormously complex question and cannot be thoroughly addressed in this brief discussion. However, we will note some basic points relevant to causation. There is a wide variety of causal forces and a great deal of indeterminacy in any historical outcome. To establish cause means to present a logically consistent and interrelated body of presumptions

about the nature of man and the operations of social reality, and to demonstrate how these principles produce a set of outcomes which is somehow necessary given a certain philosophy of history and the known behavior of all of the units—individual, structural and otherwise—of the historical experience.

The sociological perspective and, more specifically, the multiplicity of theories that are invoked by the several schools within sociology, are instruments by which historical cause may be established. Of course, the researcher must maintain a pluralistic view on the nature of causation, and must not overemphasize the deterministic element in historical sociology. The assignment of cause and the demonstration of historical conditions which necessitate a particular outcome are processes which create insight and understanding.

Assessment of the *dominant traits* or *personality* characteristics of particular historical actors is related to the general problem of cause. But there are several problems with the use of personality variables. The assessment of specific historical actors may deflect the historical sociologist from an analysis on the level of *social* units or roles. It is wrong to assume that a single consistent theory of motivation exists, or even that the actor, writing in retrospect, is fully capable of defining his or her own motives. It is further incorrect to assume that the personality characteristics of a historical actor remain consistent over the course of a lifetime, and that an assessment of personality characteristics at one stage can lend insight to another.³ Personal influence is a motivational force which is overrated as a source of historical action. Becoming a movement follower, for example, involves contact, structural conduciveness and other non-personality variables. The re-

³ Robert Dale Owen, son of New Harmony's founder, assessed the promise and personalities at the experiment in radically different ways between the time he was a participant in the experiment (n.d.) and the time he wrote his autobiography a half century later (1874). Empathy toward a lifetime of change in this individual should temper the reader's reaction to the cynicism and hostility within the latter account.

searcher should not put too great a stock in explanations which rest heavily upon the assessment of personal characteristics of individuals. At the same time, s/he should not ignore the compelling uniquenesses of historical actors. This may seem contradictory, but historical sociologists must attempt this compromise.

A final issue is the *influence of the present on the selection and interpretation of data in the past*. Gottschalk (1947:67) argues that the good historian makes every attempt possible to understand persons and events in their own setting. However, the present influences the historian's interpretation in at least three different ways. First, the researcher tends to understand another's behavior in the light of his or her own behavior patterns, and develops psychological analogues between his mental processes and those of the historical personalities. Second, the historian's choice of subjects for investigation and the selection and arrangement of data is related to the contemporary intellectual atmosphere in which he or she lives. Finally, the researcher draws on current events to form historical analogies to the episodes and developments of the past.

CONCLUSION

Historical sociology is filled with challenge and uncertainty. As Erikson (1971:76) says:

At his best, a person who works with historical documents comes to accept as a working principle the fact that the eyes with which he sees have all the defects of the age in which he lives, and that other eyes will see things differently as a matter of course; yet at the same time he accepts the conventional lore of his discipline as a provisional source of wisdom, a base from which to operate.

This paper addresses the need for a set of sound guidelines for the use of personal documents in historical research by examining this type of data in terms of four criteria: representativeness, adequacy, and reliability of the data, and the validity of interpretations. While definitive guidelines for reducing distortion and other difficulties are virtually impossible

to recommend, we suggest how these problems may at least be recognized and incorporated into the final report. Our broadest recommendation is for historical sociologists to combine thorough and critical insight into available data with an appreciation of theoretical models which underlie the discipline.

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SOCIOLOGICAL THEORY AND HISTORICAL SCHOLARSHIP

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The relationship between history and sociology was crucial to early definitions of the scope of academic sociology. But sociological theory has continued to hold to a traditional and largely outworn notion of historiography. The rise of analytic historiographies contains important implications for sociological theory. The new historiography reduces the familiar gap between history and theory, and points to new directions in theoretical work. The progress of sociological theory, then, may well require an end to a strictly sociological approach to theory.

The relationship between history and sociological theory is more than merely interdisciplinary. True, the status of historical research in sociological theory has most frequently been expressed as interdisciplinary "cooperation." But reflection upon history in sociology raises questions about the future of existing disciplinary divisions in the social sciences.

The issue of history in sociology has recently attracted much attention; literature on the subject is enormous (Eisermann, 1974:395-404). Yet the volume of this literature is matched by its inconclusive nature, leaving intact conventional boundaries between historiography and theoretical work in sociology (Jones, 1974:295). Some social historians approve of the convergence of history and sociology, but periodic pronouncements by sociologists on the utility of historical "data" have had little impact upon theory. As Martins (1974:249-254) has noted, currently fashionable sociological theories give even less attention to historical dimensions of social life than the "ahistorical" functionalism they seek to supplant. The issue of history in sociology is not a new concern in the discipline, as some have claimed (Lipset, 1968:22), but extends back to the origins of academic sociology in French and German universities. If little cumulative progress can be seen in the history of this literature, consolation can be found in its distinguished lineage.

Early Debates on History and Sociology

The rise of academic sociology in France and Germany was intimately

linked to criticism of both descriptive historiography and speculative philosophies of history. Early definitions of the scope and nature of sociology cannot be separated from the polemic between history and the new discipline.

In Germany, *Verstehen* sociology offered an explicit alternative to extreme historicism and to speculative history. The possibility of a natural science of history was firmly rejected by the neo-Kantian outlook of German scholars (Rickert, 1921:201). But Rickert's and Weber's neo-Kantianism bridged a previous distinction (e.g., Windelband, 1904) drawn between the natural sciences, which derive uniform laws by generalizing, and historical science, which emphasizes unique events. Sociology occupied the middle ground as a generalizing, natural science in the cultural realm of values (Rickert, 1921:200-205), situated midway between the search for universal laws and the search for the unique causality of single historical events. Weber and Mannheim conceive general concepts in sociology "in relative isolation from their historical incidence" (Mannheim, 1956:79; Weber, 1959:110-111, 184). By specifying the extent of this "relative isolation," theoretical statements can avoid the twin pitfalls of historical reductionism and philosophic speculation (Mannheim, 1969:174-176).

In France, academic sociology developed along positivist lines (Clark, 1973), as a natural science of society. Durkheimian sociology was thus faced with a serious challenge, which argued that empirical historical research could provide a total view of man in society (e.g.,

Lacombe, 1894; Seignobos, 1901). Protracted debates followed over this issue (cf. Sée, 1931). Durkheim and his supporters rejected the hostile identification of sociology either with a methodology of historical research or with metaphysics (Durkheim, 1900, 1970a:127; Simiand, 1903:18). Instead, they defined sociology as *the* substantive science of society, combining historical nominalism and philosophic rationalism (Durkheim, 1964b:76–77), and absorbing all specialized historical disciplines. This ‘academic imperialism’ of Durkheimian sociology (Lukes, 1973:398–405) was, in part, a reactive self-definition in response to hostile criticism by historians. To these adversaries, Durkheim left purely descriptive legwork or speculative metaphysics.

Thus, the sociologies of Durkheim and Weber were crucially delimited by their position on historical research. Historiographic generalizations were criticized as reductionist, speculative or unsystematic. The formation of general concepts was reserved for sociology. Still, Durkheim and Weber firmly thought that they had solved the duality of concrete historical diversity and abstract philosophies of history. Historical scholarship was generally regarded as an “indispensable auxiliary to sociology” (Durkheim, 1970b:154, 1958:238) and historical knowledge was the basis for theoretical work (Weber, 1959:102). From this viewpoint, distinctions between historical and sociological approaches to the study of society were bound to disappear (Durkheim, 1964a:343, 1909:157; Weber, 1959:176–177). The opposition between ideographic historiography and nomothetic social science would therefore become an internal dialectic within sociology.

However, Durkheim’s and Weber’s vision of a sociology immersed in history was precluded by the very development of academic sociology. The crystallization of professionally defined disciplinary subjects literally demanded the separation of atheoretical history from ahistorical theory. These academic divisions conveniently excluded historiographic issues from the domain of sociology. Instead, the conceptual vocabulary of Durkheim and Weber was uncritically adopted to constitute the scope of sociological thought. For

some time, critical debates on history and sociology were ignored, and they receded into the “pre-scientific” mists of the discipline.

Anglo-American Sociology and History

Rising sociology departments in Anglo-American universities continued the separation of history and theory. This is hardly surprising given the familiarity of early American and British sociologists with their German counterparts. To the founders of American sociology, sharp distinctions between historical studies of unique events and the formation of sociological laws seemed eminently logical (Giddings, 1901:8–9; Park, 1921:406, 411; Ross, 1903:194–197; Small, 1904:292–294; but cf. Znaniecki, 1934:23). By the 1930s, some observers called for cooperative work leading to historically specific theories (Ginsberg, 1932:42–43; Postan, 1971:16–21; Salvemini, 1939:9–10). But questioning the divisions between historical scholarship and sociological theory was far less conspicuous than *ad hoc* recommendations for more interdisciplinary “contacts.”

There was renewed interest in the status of history in sociology after World War II in which the theme of interdisciplinary cooperation still prevailed. The ideographic/nomothetic distinction between history and sociology provided the framework for commentary on the need for interdisciplinary work (Wolf, 1959). Even more recent writings about history and sociology (cf. Cahnman and Boskoff, 1964:2–3; Holloway, 1963; Runciman, 1970; Sorokin, 1962; Wilson, 1971) invariably conclude with pleas for more interdisciplinary cooperation (Wehler, 1973:11). Moreover, such discussions hold firmly to the traditional Rankean view of historiography: the study of history reveals the past event “*wie es eigentlich gewesen*” (the way it really was). History is accordingly seen as a field of empirical verification for general theories (Collins, 1975:36–37; Fisher, 1960:158; Lipset, 1968:30; Sorokin, 1962:240), since it concerns the *real* events of the past, singular in their space-time existence and exhibiting a serial

causality (Aronson, 1969:330-331; Gibson, 1960:179-186; Halpern, 1957:4; Lipset, 1968:22-23; Smelser, 1968:37; Thrupp, 1957:11-16).

This traditional view of history and historiography should be seen in the context of an intellectual climate of pervasive positivism. The antithetical relationship between history and sociology was simply presumed to be a relationship between empirical data and generalization. As Habermas (1967:40) observed, positivist presuppositions created opposing arrays of logical and methodological canons that delineated seemingly inevitable boundaries between history and sociology. This situation arose out of well-known positivist critiques of "historicism" (Hempel, 1965; Popper, 1960) and of evolutionary and teleological currents in social theory (Bock, 1956; Nisbet, 1969; Teggart, 1960). A positivist mode of historical explanation requires the reduction of history to eventual history. By reducing history to a succession of singular space-time events, the historical actuality of society is transformed into empirical data. Historical explanation can then assume a scientific character, using inductive generalization and counterfactual tests, precisely because singular events are subject to quantitative representation. But there can be no link between historical work and concept formation. History, then, becomes a source of retrospective confirmation or refutation of general theory.

The insularity of American sociology also helped to promote a polar view of history and theory. Objections were confined to marginal reaches of the profession (e.g., Mills, 1959:143-164) or unknown because they were rooted in foreign cultural and philosophic traditions (e.g., Braudel, 1958; Gurvitch, 1957, 1964).

The Current Situation

The positivist reduction of history to the history of events is still prevalent today. This view is supported by a variety of "metatheoretical" distinctions based on notions of what practicing historians and sociologists do. It is argued that history deals mainly with the past while sociology deals mainly with the present, or that his-

torians use primary sources while sociologists use secondary sources, or that different methodologies and research techniques characterize the two professions. Certainly the rise of quantitative historical research has undermined some of these distinctions. Some observers (Erikson, 1973:18; Tilly, 1970; Wolf, 1959) have objected to such distinctions, which invariably define history as an ideographic science and sociology as a nomological science.

The circularity of these "metatheoretical" differences is plausible only within a narrow positivist horizon, in which existing disciplinary divisions are justified by practices associated with the separated disciplines. A scientific discipline is surely not created by methodological canons or research techniques. Rather, selection of research methods should be dictated by the conceptual relations that constitute a particular science. If a polar view of history and theory is to be upheld, then a theoretical argument must justify an ideographic/nomothetic cleavage in the social sciences.

Phenomenology and History

Recently, a number of "phenomenological" sociologies have arisen in explicit opposition to positivist sociology. According to their proponents, the conflict between positivism and phenomenology is the major issue in contemporary theory (Bruyn, 1966; Douglas, 1970; Silverman, 1973; Walsh, 1973). But debates surrounding this conflict leave untouched the problem of history in sociological theory. The lack of historical depth in current phenomenological sociology is just as unsatisfactory as the positivist separation of history and theory. As one phenomenological proponent has noted, ethnomethodology, existential phenomenology and neo-symbolic interactionism are linked precisely by the excessively narrow temporal parameters of their social-psychological concerns (Glass, 1972:4). Indeed, the temporally truncated nature of phenomenological research is, in a substantive sense, compensated by its 'inflationary cognitivism' (Martins, 1974:253).

It is difficult to see how preoccupation

with effervescent contacts, nuances of daily life and tacit conventions could be other than ahistorical. But phenomenological research is not necessarily tied to a self-enclosed microsociology, nor is microsociology tied to ahistoricism. Writings by Gurvitch (1964), Halebsky (1976:120–123) Lefebvre (1971a), Sennett (1976) and Sennett and Cobb (1973) show how microsociology can be historically grounded and phenomenology wedded to larger structural issues. This is also evident in the Frankfurt School's work towards a phenomenological Marxism, which has recently been developed by Schroyer (1973), Wellmer (1971) and contributors to the journal *Telos*. But, for the most part, current research in phenomenological sociology remains engrossed in its ahistorical preoccupation with the cognitive processes of daily life.

Theoretical statements of phenomenological sociology do recognize the historicity of social life, but only in a trivial way. Temporal dimensions of social life are viewed in terms of a cognitive dynamic: history is a subjective field of reality construction. The basis of this reality construction results, in the first instance, from the existential rupture between biography and history (Berger and Luckmann, 1967:58, 93; Schutz, 1967:213–214). Historicity acquires the character of a subjective drama which must constantly reconcile man's inherently "free" intentionality with pre-given, sedimented layers of social meanings. Thus, the determinate nature of history is transformed into its opposite: a universal verity of the individual's life cycle. Historical scholarship becomes a study of conceptions invoking—explicitly or implicitly—existential historicity (Lyman and Scott, 1970:190), and historical objectivity refers to a wholly abstract relationship between predecessors and the present.

Structuralist Marxism

Marxist sociology might plausibly be expected to provide some unifying admixture of history and theory. Yet the most fashionable new theoretical work in Marxism is also the most rigidly ahistorical.

Structuralist varieties of Marxism have consistently denied the relevance of the problem of history for theory. And what began with disavowal of genealogical or teleological schemas in Marxism (Althusser, 1969; Althusser and Balibar, 1970) has more recently led to the wholesale denial of history. Under the banner of "the autonomy of theory," this variety of Marxism rejects "the notion of history as a coherent and worthwhile object of study" (Hindess and Hirst, 1975:321). Investigations into historical dimensions of social life are seen to yield only triviality and to fulfill purely literary functions. In this view, Antonio Fraser's biographies represent the essence of historiography. Neither a real nor a purely conceptual succession of social formations has any place in this structuralist clarification of concepts used to construct modes of production. Instead, the evaluation of concepts is wholly contained within the realm of theory (Hindess and Hirst, 1975:308–320; Poulantzas, 1973:12).

Other varieties of Marxism have resolutely opposed this antihistorical thrust, equating it with sectarian scholasticism (Lefebvre, 1971b). Some theorists have remarked on the similar antihistorical bias in both systems theory and structuralist Marxism, suggesting that a truly critical theory is inseparable from a historical standpoint (Post, 1974:154–155; Schmidt, 1970:209). Moreover, the most important recent works in the Marxist tradition are precisely those dealing with substantive historical issues (e.g., Anderson, 1974; Birnbaum, 1969; Braverman, 1974; Foster, 1974).

Toward Theoretical History

For some social scientists, a rigorous division (whether disciplinary or epistemological) between historiography and concept formation is untenable. A growing international consensus approves the convergence of social history and historical sociology (Braudel, 1958; Briggs, 1968; Hobsbawm, 1971; Tilly, 1970; Wehler, 1973). Various analytic historiographies span, in practice, the gulf between history and sociology. Consider, for example, recent work by the *Annales* school in

France, represented by Braudel and Labrousse (1970) and articles in honor of Labrousse (1974); or consider work in Britain by the Cambridge Group on population and family history (e.g., Laslett, 1977). Similar trends exist in the United States (Moore, 1966; Thernstrom, 1973; Tilly, 1964; Wallerstein, 1974) and in Germany in debates between Marxist and non-Marxist scholars over the sociology of the reformation (Wohlfeil, 1972, 1975).

As traditional historiography—concerned with chronologies of events—declines in relation to new analytic historiographies, disciplinary distinctions become increasingly superfluous. Analytic historiography obviates the ideographic/nomothetic cleavage between history and sociology. The eclipse of traditional by analytic historiography has been largely animated by shifting interests in historical work, moving away from administrative, diplomatic and political histories toward socio-economic history. Thus, as has long been predicted, the rise of social history has implicitly undermined the separation of ahistorical theory and atheoretical history (Durkheim and Fauconnet, 1903:486–487; Jones, 1974:270–271; Ludz, 1972:11). But it is important to note that this development has primarily been associated with practical research concerns of social historians and historical sociologists (Tilly, 1970). Its explicit impact upon systematic theory has yet to be seen.

Implications for Theory

The rise of analytic historiography contains strong implications for sociological theory. Though these implications have yet to be realized, their probable outlines can be sketched as follows. A true synthesis of historical scholarship and sociological theory will emerge when concept formation is informed by research into the historically determinate nature of society. That is, the process of singling out structures from the flux of history cannot be detached from conceptualizing typical elements and relations within any particular structure, past or present. Abstractions in historically grounded theory would necessarily be *determinate* ab-

stractions (Colletti, 1972), not the *general* (hence ideal) abstractions about society which can be variously assembled to create the “sociological thought-machine” (Adorno, 1967:42).

These implications are by no means limited to purely macrosociological concerns. There are many histories encoded in different time scales (Braudel, 1958), ranging from short-term histories of events, to long-term structural histories, to intermediary conjunctural histories concerning cycles and crises. Reconstruction of these diverse rhythms and tempos of past time can historically ground theoretical accounts of continuity and change at all levels of social reality. Micro- and macro-theoretical work need no longer address two severed facets of social life, but, instead, can analyze the same reality within two different time frames. Historically grounded theory means that concepts emerge from the analytic problem of history: ordering the past into structures, conjunctures and events. History and theory can thus be simultaneously constructed in the effort to relate a plurality of historical tempos. It is the lack of attention to this problem of taking time seriously that vitiates current modes of theory construction in sociology. In addition, historically grounded theory is necessary for understanding the present by revealing the historical actuality of our own society.

These remarks indicate that theoretical work cannot be detached from substantive work recognizing the irreducible historicity of its subject. Synthetic research with an inward-directed concern for systematic sociology will not do much to advance theory; indeed, it is largely responsible for our current theoretical malaise. The progress of sociological theory may paradoxically require an end to a strictly sociological approach to theory. The convergence of historically informed theory and theoretic history promises a viable, perhaps necessary alternative to current efforts to derive new “paradigms” by theoretical exegesis. The fruitful nature of this alternative can be gauged by reflection upon the source of the most enduring conceptual themes in sociology. Fundamental polar concepts of classical sociol-

ogy (e.g., *Gemeinschaft/Gesellschaft*, status/contract, military/industrial, mechanical and organic solidarity) all derived from historical interpretations of the transition from feudal to capitalist society (Mills, 1959:152–153).

What is presently required, then, is a recognition of the historicity of theory in a double sense: (1) theoretical work is, explicitly or implicitly, historical work; (2) the theorist necessarily expresses historically conditioned interests, be they intellectual, moral or political. The all-engrossing attention currently given to synthesizing hybrid theories—especially evident in “humanistic” sociology—poorly serves intellectual, moral or political interests. Such interests would be better attended by developing historically grounded theory able to assess the present as a result of the past in order to deal intelligently with the future. The salutary role of historical scholarship in sociological theory, therefore, would not only apply to our scholarly interests, but to wider human interests as well.

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GEOLOGY AND SOCIOLOGY: PROBLEMS AND PROSPECTS OF THE 'SOFT' SCIENCES*

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The American Sociologist 1978, Vol. 13 (May): 122-126

Geology and Sociology: can there be any dialogue between such apparently diverse sciences? I claim that there can be, and that sociologists would benefit from the relationship.

There was a time, only a century ago, when it was possible to be both an earth scientist and a social scientist. The two disciplines are still quite similar in their approaches to scientific method. Both rely heavily on models and statistical tests. Occasionally, research in the two fields appears to resemble the experimental approach of the hard sciences. In general, however, geology and sociology are basically inductive and rely more on survey-type research than on experimentation. Both deal with essential independent parameters which cannot be controlled. Geology has its vast scale and geologic time; sociology its human behavior and potential reactivity. Geologists and sociologists both use statistics to solve sampling and hypothesis testing problems. The Statistical Package for the Social Sciences (SPSS, Nie et al., 1975) is standard equipment for both.

Scientific Development

But geology and sociology differ sharply in the goals that underlie their research efforts, and consequently in their methods. Since Durkheim, the traditional goals of sociological research have been *description* and *explanation*. This has fos-

tered a methodological approach in sociology that concentrates on model testing (as opposed to model building) and a methodological emulation of the hard sciences, particularly physics (as opposed to the soft sciences of biology and geology). Geology also has a history of descriptive research, but contemporary research explicitly aims for *prediction* and *application*. Geology has discovered a master paradigm by means of an alternative focus on inductive model building.

After nearly two centuries of disorganized debate and data overkill (Cannon, 1960; Gillispie, 1951), geology is currently undergoing a "revolution" (Kuhn, 1962) that has brought its several subdisciplines of petrology, paleontology, structural geology, geophysics, and geochemistry together under one unifying paradigm (Dewey, 1972). This master paradigm, the "New Global Tectonics" (Hallam, 1973), holds that the outer crust of the earth consists of a dozen or so plates which are moving relative to each other at rates of a few centimeters per year. Although inexact versions of this idea have been proposed, debated, and debunked for half a century, imaginative new reconstructions of the model emerged and became overwhelmingly persuasive. The really valuable aspect of Global Tectonics, and of any model, is its pertinence to a multitude of specific problems; that is, one master paradigm provides the basis for model building on the local level. Further, the combined results of the specific tests of local models has led not only to support of the larger model, but has resulted also in much more *efficient* application of scientific effort, to both theoretical and applied problems. Geologists approached this research inductively, and only subsequently proceeded in a deductive fashion. For example, if plate tectonics describes the

* I am indebted to J. Kenneth Osmond and several of his graduate students who consulted with me on current activities in the field of geology. These geologists, of course, should in no way be held responsible for my comparisons of or contrasts between the two sciences. I am also grateful to the staff of *The American Sociologist* for detailed and knowledgeable critiques of earlier versions of this paper, and for exemplary editorial assistance.

way that the earth's crust is behaving, then it follows that earthquakes will occur where the plates are colliding.

Model Building

How can the geological revolution contribute to sociology? Three major, somewhat interrelated, emphases of geological methodology appear relevant: (1) a major research goal of inductive model building; (2) a trained ability to be open to and deal with a variety of models; (3) a constant search for methods appropriate to the study of change over time.

Induction In many ways the methodological emphasis in geology resembles what we refer to in sociology as "qualitative" methodology. In a large portion of geological research, the goal is discovery, the method is multiple observations and comparisons, and the result is model building. Geologists are totally perplexed when informed that Glaser and Strauss's (1967) proposal of "grounded theory" is controversial in sociology. The discovery of a model (i.e., theory) from data that results in a very close, systematic, and specific fit of model and data is the only logical way for a geologist to proceed. Moreover, the geologist uses this method with *quantitative* data. What sociologists might term "data dredging" is accepted and approved procedure for the geologist, who is often not testing a model but attempting to build one.

Of course, geologists also test models, but there is more of a balance in the geological literature of model testing and model building than is the case in sociology. To geologists, model building often precedes and always goes along with model testing. As noted in the example of the plate tectonics model, after the discovery of a grand paradigm geologists continue to build and, concurrently, to test local or more specific models. The major purpose of this model building is to discover and/or to recognize significant variables. Sociologists are prone to test relationships without adequate modeling (Duncan, 1975; Leik and Meeker, 1975).

Variety of Models Because both geology and sociology often focus on patterns that are "invisible" in the natural world (for example, societal "institutions";

geochemical "diffusions"), models are especially useful. According to geologists, the skill most essential to model building is creative/imaginative *visualization*: "visualize first; verbalize second." This is quite apparent in the very different formats of sociological and geological journal articles. Sociologists typically use tables and statistical tests as evidence for conclusions, while geologists draw upon maps, scales, graphs, diagrams and visual aids of all kinds. Such displays permit rapid and persuasive support or rejection of inferred relationships.

Krumbein (1969) has categorized several types of geological models, among them the concept model, diagrammatic model, process-response model, statistical predictor model, deterministic model, and stochastic model. The classification itself is not so unique; the significant aspect is that Krumbein and his geologist readers are able to visualize example after example of each, plus readily recognize new situations where they can be applied. Students are trained and expected to be model oriented.

The sociologist may object to this analogy between the sciences because, s/he assumes, geologists work only with spatial, physical, geographic visualization. True, geologists work constantly with spatial problems which require three dimensional visualization (such as the orientation of folded strata or the arrangement of mine shafts and adits). But they work also with variations of spatial models involving a single variable as a function of geographic position. This is a three dimensional problem with the independent variables consisting of position on an x-y plane, and the dependent variable often being an abstraction. From such diagrams it is an easy transition to other diagrams with three dimensions which do *not* involve spatial parameters.

Because of their familiarity with spatial problems, geologists are comfortable with models involving three dimensions even though the variables themselves are not spatial. In fact, physicists and chemists often use models involving four or more dimensions—something geologists as well as sociologists are unaccustomed to doing.

Change over Time Because "time" is a central variable to geologists, they are on the alert to discover and apply innovative methods that allow the study of change over time. But the strongest area in geological methodology is one of the less developed in sociology. A major problem in sociology is the limited number of good models that deal with social change. However, there are recent examples in the sociological literature (Snyder, 1975; Hernes, 1976; Land and Felson, 1977) that indicate an increasing attention to modeling change processes.

From a geological perspective, one of the most conspicuous faults of sociological models is the assumption of *linearity* (see Leik, 1977). There are at least two reasons why linear models are so widespread in the sociological literature. First, linearity represents a reasonable assumption when comparing across cases from cross sectional data (data collected at one point in time). Second, if all relationships are assumed to be linear (and additive), then a relatively simple set of equations may be utilized. The relatively unsophisticated researcher need not even write out these equations but can rely on a computer package (like SSPS) to perform the analyses.

However, the unquestioning application of linear equations is not only atheoretical but is possibly an important factor in the very small amounts of variation that sociologists are able to explain in models of change or developmental processes. The low explanatory power of many of our models *could* be due to the fact that linearity does not obtain (Leik, 1977); however, many sociologists simply add variables to their models (with little theoretical rationale) rather than explore the possibility of alternate forms of the relationships.

Leik (1977) offers a good example of sociological model building that eschews the assumption of linearity. His model illustrates that not only may associations be nonlinear, but they may also *change* forms over time (see Snyder, 1975, on this latter point). The focus of Leik's study is the relationship between duration (years) of marriage and probability of divorce. He finds that, at the

outset, the association can be represented by a power curve (an exponential growth process). Beyond year four, however, the form changes to a declining exponential curve. Leik then derives a "general" curve and, subsequently, asks what changes occur when one considers additional variables. With the addition of "age at marriage" the model gains in predictive power, and Leik demonstrates how this methodological process could continue. Of course, there are other examples in sociology (Spilerman, 1971; Stoltzenberg, 1975) that indicate the utility of nonlinear specifications.

There are many nonlinear forms other than power and exponential curves. Geological research offers numerous examples of dealing with change over time that sociologists might profitably explore. The following illustrations are suggestive, not exhaustive.

The concept of "half-life," as used in geological age studies, involves an exponential distribution that might also be applicable to certain sociological research topics. The exponential function is:

$$N(t, \lambda) = N_0 e^{-\lambda t}$$

This equation is derived from a differential function that can be approximated by:

$$\Delta N/N = -\lambda \Delta t$$

Stated verbally, the relative change in some phenomenon (N) is constant for relatively short time intervals (Δt). However, the absolute change is not constant but changes logarithmically as the phenomenon itself changes. The constant factor (λ) is negative for a decreasing value of N, and can be replaced by $0.693/T$, where T is the "half-life" of the phenomenon, the time required for the value of N to decrease by half.

Applying this to a sociological phenomenon, it appears possible that certain social roles which were once highly functional (i.e., essential to group well-being) but which have outlived their usefulness would decay or degenerate in a similar manner. We could hypothesize, for example, that the change in traditional sex role behavior might involve a lambda (the

change term) of 0.1 per generation. Then the half-life of this behavior becomes $0.693/0.1$ or seven generations. Further, after 40 generations there would still be a few (approximately 1%) cases of traditional behavior remaining. It is also likely that there are half-lives of initially attractive, but in the long-run not useful, social theories and research methods. It takes a few generations of professors and students for these to disappear.

If λ is positive, the phenomenon being described *increases* without limit and the concept of "doubling time" replaces "half-life." The growth of industry, technology, and resource utilization has been cited ("limits to growth") as such a function with an ominously short "doubling time." "Doubling rates" have received some attention by social scientists. Price (1963) presents evidence, for example, that the number of journals in science has increased by a factor of ten every half-century since 1790, and the number of abstracting journals has followed exactly the same model, multiplying by a factor of ten every half-century. Bell (1964:849), over a decade ago, lauded Price's work on logistic curves as a possible breakthrough in studying processes of social change and predicting their outcomes.

Cyclical phenomena illustrate another type of change process dealt with by geologists. For example, the geological record shows that the pattern of climate over the last few million years has been cyclical, with ocean level (the best indicator of continental ice volume) returning to approximately the same place repeatedly. However, the periods of cyclical fluctuations have been episodic. Only during a few relatively brief periods of geologic time have glaciations been possible. Sorting out the number and period of distinct cycles which are superimposed (spectral analysis) requires many data points and is never "complete." New data can always add to the number of cycles recognized or modify the time periods of those already recognized.

In sociology the idea of a cycle has been taken more as a metaphor than as a research model. Available evidence, however, suggests that many social phenom-

ena may change in a cyclical fashion. For example, rates of premarital intercourse, rather than increasing linearly (as the literature often suggests), appear to have been relatively low in the nineteenth century compared not only with the twentieth century but with the eighteenth as well. Further, household composition (in terms of one to three generation households) also shows ups and downs over time in a cyclical mode (Gordon, 1978). Sociologists, if freed from linear thinking, could find examples of cyclical change in any substantive area.

The relatively new mathematical method of "catastrophe theory" (Thom, 1975; Zeeman, 1976) allows the researcher to quantitatively model discontinuous and divergent phenomena. Catastrophe theory always applies to the change in one variable relative to at least two others, and in strictly nonlinear fashion. Differential equations have been traditionally used to describe a process in which the dependent parameter behaves as a smoothly changing function of the independent parameters. In contrast, catastrophe theory allows the researcher to model processes for which the dependent parameter has abrupt changes in value and assumes one of several values for a given value of the independent parameter. Geologists (Lantzy, Dacey, and MacKenzie, 1977) have applied this model to the Permian extinction of marine invertebrates. They conclude that a reduction in oceanic salinity was a more significant factor in the extinction of marine invertebrates than was a reduction in the area of shallow seas. In sociology it appears possible that, for example, new insights on abrupt social change (such as conflict and collective behavior) could be gained by use of the catastrophe models. However, the point is not that catastrophe theory, strictly applied, necessarily represents a breakthrough in approaches to the kinds of complex processes characteristic of the soft sciences. Many geologists are also skeptical. Rather it is that geologists are more readily able to "try it out"; that is, to deal with the discontinuities and loops on which the theory is based.

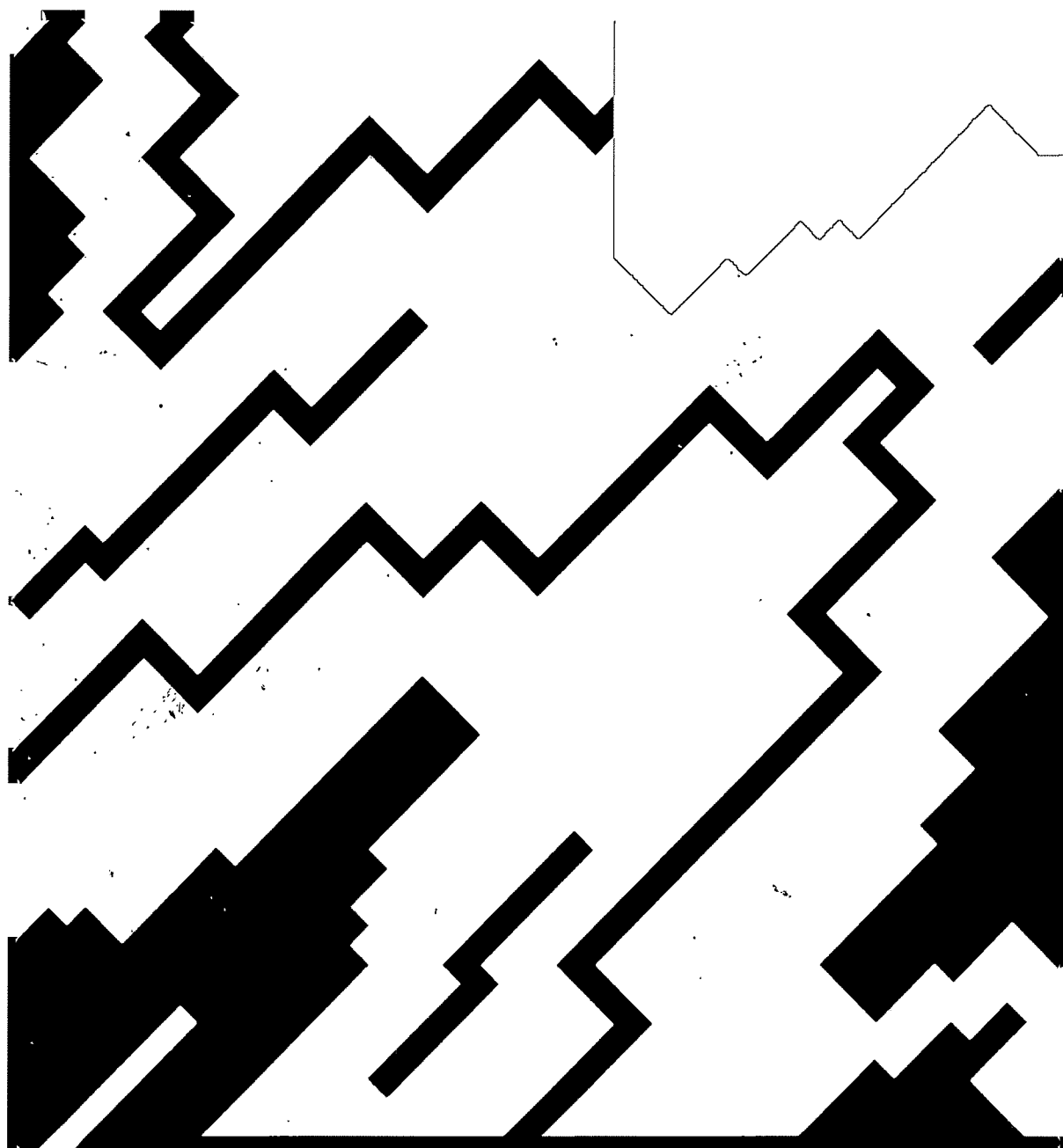
The above examples demonstrate the *variety* of essentially nonlinear models

that geologists use to study change processes.¹ A major point, for our sociological comparison, is that this methodological variety can be tapped by the average geology graduate student as well as the practicing geologist. Sociology may benefit not only from the methodological emphases of the "soft sciences," such as geology, but may also go further to discover that human interaction shares at least some of the properties of interaction of other physical forms.

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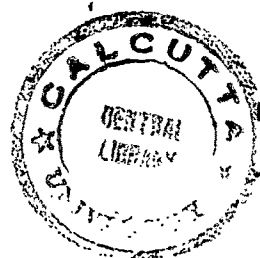
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¹ The variety of geological models in current application is overviewed in a recent article in *Geotimes* (Merriam, 1978). These include discriminant functions, Zipf's law, trend-surface analysis, search strategy, conditional probability, decision theory, characteristic analysis, dynamic-cluster analysis, etc.



The American Sociologist

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EDITOR'S PAGE

We sociologists have not been indifferent to ethical considerations in the practice of our profession. There have been books published on the topic, occasional articles in our journals (including *TAS*), exchanges in our professional media, and, most recently, regularization of concerns about protection of human subjects in non-sponsored research. I have the feeling, however, that we have attended less to ethical issues than have some of our colleagues in cognate disciplines, both in terms of classroom and published discussions and in terms of formal attention by organs of the ASA. The articles in this issue show that we cannot complacently ignore ethical questions; the commentary and debate on the articles show that we are far from agreement on how we should respond to them.

Three submissions do not constitute a trend, but I suspect that the independent submission of the three articles which constitute the core of this issue within a period of about six weeks was not fortuitous. All three authors wanted to tell us about past and projected events that they consider to be "professional concerns of sociologists as a social collectivity." Stephenson feels he has been stung; Cassell feels we have been cavalier and callous in our treatment of those we study; and Bond is concerned about the erosion of the protection of studied populations. The topics addressed in the three articles do not exhaust those which should concern us. The perspectives represented are quite different. We hope that the articles and the commentary will stimulate broader discussions on both the specific issues and on more general matters of professional rights and obligations.

As we have done in earlier "exchanges," we sought commentary from a wide range of individuals who we hoped

would represent a range of both disciplinary and ideological perspectives. As in the past, we found that a number of persons declined to participate on a variety of grounds. One distinguished anthropologist told us that he had done a lot of talking about professional ethics throughout the years, and had finally decided to keep his mouth shut about it, on grounds that these are actually political, not ethical questions, and that he would prefer to "stay out of it." A similarly distinguished psychologist declined on grounds that he could not risk having his personal commentary interpreted as an official position. Similar disclaimers were made by several foundation officers. Still others whose comments were sought stated that their positions were already on the record. Some, of course, were busy with their own research and with attempting their own resolutions of these sticky problems.

It is clear from the comments we are publishing that there is no consensus. Indeed, some of our commenters feel that the issues themselves are spurious, either because (as scientists) we should simply "get on" with our work, or because social scientists have little influence for either evil or good in the "real" world. Others agree that there are serious problems, but are unsure about effective (and ethical) resolutions. Still others, finally, have quite concrete suggestions about at least partial solutions.

I hope that future issues of the journal will contain further contributions to the discussion of both the specific issues addressed in these pages and other ethical concerns. I should note that while we are concerned about the accuracy of facts as described in both articles and commentary, we have no way of checking factual

Continued on Cover 3

A processing fee of \$10 is required for each paper submitted; such fees to be waived for student members of ASA. This reflects a policy of the ASA Council and Committee on Publications affecting all ASA journals. It is a reluctant response to the rapidly accelerating costs of manuscript processing. A check or money order, made payable to the American Sociological Association, should accompany each submission. The fee must be paid in order to initiate the processing of the manuscript.

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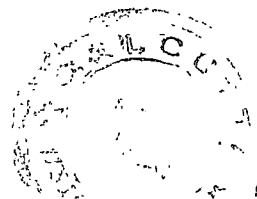
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THE CIA AND THE PROFESSOR: A PERSONAL ACCOUNT*

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The revelation that the CIA has covertly funded research through ostensibly respectable foundations and granting organizations raises a number of considerations relevant to sociologists and sociology. Most of this research was professionally legitimate, and those engaged in it did not know of the CIA sponsorship. Nevertheless, there are serious questions concerning the real or potential consequences of secret funding. In this paper I will examine some of these consequences based on a personal experience and suggest some ways for insuring the integrity of research.

On the evening of 25 August 1977, I received a telephone call at my home from a reporter on a local newspaper. That was how I first learned I had engaged in research secretly funded by the Central Intelligence Agency. For twenty years, I had assumed and had every reason to believe that the research in which I participated was ethically conducted and that the source of its funding was what I believed it to be. The research in question was a study of refugees from the Hungarian uprising of 1956, and funding for it was assigned to Rutgers, the State University of New Jersey. The day following this call, a front page article appeared in the newspaper and was headlined, "'56 Study May Be CIA—Rutgers Link." Subsequently, other reporters got in touch with me; the one from the *Washington Post* produced a six-column report headlined, "Rutgers Received CIA Funds to Study Hungarian Refugees."

In the meantime, I learned that the President of Rutgers had received a letter on 12 August 1977 from Anthony A. Lapham, general counsel for the CIA, informing him that Rutgers was among a number of institutions at which "some portion of CIA-sponsored research appears to have been performed or with which one or more individuals performing some aspects of it were affiliated." The letter indicated that the institutions or in-

dividuals appeared to be involved with project MKULTRA, a CIA project designed to identify, test, or study "materials and methods useful in altering human behavior patterns." The letter further stated that "most of the research did not involve such testing but rather only far less controversial investigations into aspects of human behavior and its determinations" and that "in many cases, the individuals or institutions, or both, were apparently not informed that the research was connected in any way with the CIA."

Upon receiving this letter, the President indicated that it was the first knowledge he had that the University had been involved in such research, and the University requested that the CIA furnish any documents relevant to the matter. The documents (described in a cover letter as "all available materials concerning Project MKULTRA which related to Rutgers University") were sent to the University. They consisted of several pages of receipts, vouchers, and accounts relating to a grant to the University, an authorization of the grant, and a description of the research objective and procedure to be undertaken. The names of institutions or organizations other than Rutgers, all individuals, and all CIA employees except those previously acknowledged publicly by the CIA were deleted from the documents. The research was identified as "MKULTRA, Subproject 69," and it was stated that the study "would be financed through" a source, whose identity was deleted, which "will act in the capacity of a cover organization."

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The CIA Project

MKULTRA, the code name for the general CIA project, first came to public attention in 1973 through the investigation of the Senate Subcommittee on Intelligence. Many of the documents concerning the project had been destroyed in that year, apparently by order of the then CIA director. However, in July 1977 it was announced that seven cases of records were discovered in the CIA archives that had been overlooked in the 1975 and 1976 investigations of two Senate committees. In August 1977, 10,000 additional documents were found in what was described as a routine review of inactive CIA records, and related material turned up in a West Coast college library. In that same month, Senator Kennedy's subcommittee of the Senate Human Resources Committee held a hearing in which Admiral Turner is reported to have said that the CIA had secretly supported research at 80 institutions, including 44 colleges or universities, hospitals, prisons, and pharmaceutical companies. These and similar sources indicate that CIA-sponsored research or CIA investigations were conducted on behavioral control and other matters for more than 20 years under the code name Bluebird, changed to Artichoke, and eventually known as MKULTRA—MKDELTA.

It is uncertain which of those sources occasioned the letter to the President of Rutgers and the telephone calls I received from reporters. It is clear that some of the reporters knew names and places that were deleted from the documents sent to the University. In at least two newspaper reports, information that I had not known was published about the research and some of its personnel. Most of the reporters knew that the source of the grant to Rutgers was the Society for the Investigation of Human Ecology, formerly located in New York City but no longer in operation.

The "Cover"

The Society was first made known to me in 1956 by a colleague who had learned that a study of Hungarian refugees quar-

tered at Camp Kilmer (a deactivated World War II facility) was being contemplated. In a brochure I received at the time, the Society was designated as "a non-profit organization incorporated May 25, 1955 pursuant to the Membership Corporation Laws of the State of New York." It was described as "an outgrowth of the work of the Human Ecology Study Program at Cornell University Medical College which has been conducted over a period of 25 years." It was further stated that, "In 1955, grants from beneficiaries of this study program enabled the establishment of the society with the purposes as described." These purposes were described as supporting and conducting research on human ecology in its social, cultural, psychological and physical aspects; investigating patterns of human adaptation to the environment and how they affect people's health, behavior and emotions; disseminating knowledge in these matters through lectures, seminars, and publishing; and giving fellowships, scholarships, grants, and donations to individuals and groups for these purposes. Eminent professors of medicine, law, psychology and psychiatry from major medical centers and universities were listed as directors and officers of the society.

The Research

The plan of the Hungarian study was for a team of specialists in varied fields, including medicine, psychology, psychiatry, anthropology, and sociology, to examine a selected sample of the refugees. A central purpose of the study was to investigate the source and effects of stress, disaffection, or alienation on individuals and groups, how people adapt to such experiences, and the consequences of these adaptations for those involved in them. It was thought that the presence of the refugees offered a unique opportunity for such investigations.

The sociological research involved an open-ended questionnaire, which served as a guide for extended, personal interviews. It should be clearly understood that the design of this questionnaire was not dictated by the Society, its personnel,

or other persons working on the team of investigators. Nor were any of these people present during the interviews, most of which took place at the barracks at Camp Kilmer. In cases where the person interviewed did not speak English, interpreters were provided, but there was not then nor is there now any reason to believe they knew of any relationship between the research and the CIA. It was understood, and there was every reason to believe, that the Hungarians studied participated voluntarily, and were selected so far as possible to include cases from different age, sex, marital, religious, occupational, economic, or regional categories.

Over a period from early January to late May 1957, 69 sociological interviews were conducted. These interviews were coded by number and became part of the cases also examined and interviewed by the other team members. Most of the other work took place at the Society center in New York City: transportation for the Hungarians was provided to and from Camp Kilmer. As convenience and accessibility of respondents dictated, sociological interviews also took place at the center. An additional seven sociological interviews took place in England. During this research, periodic meetings were held at the center at which participants in the study reported progress, planned for examination of further cases, exchanged findings, and discussed ideas, hypotheses, and data relevant to the purposes of the joint research.

It was understood that each participant in the research was free to report his or her findings. There was no indication that the research was "secret" or that the results of it were "classified" in any way. Papers on the sociological research were presented at the annual meetings of both the Eastern Sociological Society and the American Sociological Association. Another paper was given at a seminar on "The Hungarian Revolution of October 1956" at Columbia University. This day-long seminar was sponsored by the Society and brought together scholars and other interested persons from Institutes, Universities, and Agencies who presented papers or engaged in discussion following them.

Both the papers and the discussions were published by the Society and made available to the public. Still another paper was given at Cornell University Medical College in one of a series of open meetings presented there. I still have in my possession two file drawers of research-related material, which has been made available over the years to students and scholars who have requested it.

As this brief sketch of the source and nature of the research attests, I did not know the research was connected with the CIA in any way, the sociological research was entirely undirected by this source, and the material from it was unclassified. I have tried to convey these facts as inquiries have come to me. While there is some personal satisfaction in my being able to give assurance in this case about University policy and my professional integrity, the matter cannot be closed there. Experiences such as this occasion a number of considerations that must concern the individuals involved, their colleagues, the universities they serve, the professions with which they are associated, and the human subjects of their research.

Consequences

When I first heard of the role the CIA had played in the research I participated in, my feelings were mixed. On the one hand, I felt offended and resentful, if not actually angry. I had "been had," and by people I respected and with whom I had enjoyed a congenial and stimulating association. On the other hand, in view of the nature of the sociological data and its undirected and unclassified status, the idea that the CIA was involved and the Society was its "cover" assumed a cloak and dagger staging closer to comic opera than serious drama.

As events unfolded and I was able to reflect upon them, it became evident that more was involved than my personal feelings. I knew nothing about who was responsible for the CIA nexus, what other involvements the Society may have had with the CIA, or who in the Hungarian study may have known about the sponsorship. The Society had disbanded and col-

leagues who worked with me at the University had long since departed. I was apparently the convenient source of information for reporters. In this situation, I found myself constrained in mentioning names of individuals or organizations involved when inquiries were made. While I wanted to be as frank and open as I could, I did not want to taint innocent people or speak for them, knowing that reporters can seldom follow all leads but that names might well be mentioned in their articles. Because very serious charges have been made against research sponsored or conducted by the CIA, any mention of individuals or their affiliations was likely to raise suspicion and doubt, which may have been quite unfounded.

My own experience with public reporting of this affair has been reasonably good. My disclaimers usually have been reported, although prefaced with "Dr. Stephenson claims" or "says" or "denies," which leaves some room for doubt. Furthermore, the nature of the research is seldom detailed sufficiently in the press to separate it clearly from the more "controversial" and seriously reprehensible work of the CIA. Therefore, I found myself on the defensive, and while one need not be paranoid about it, it is something more than a nuisance. The University also has been compromised in some degree. The President has said that he was "greatly shocked and disturbed" that Rutgers might have engaged unknowingly in CIA-sponsored research. In 1966, the University Research Council and the Research Administration Board had passed resolutions recommending the rejection of research that could not be freely published. Although there is legitimate controversy about just where lines should be drawn in these matters, the fact that the funding of the research was secret, if not the research itself, places it in question.

Members of the professions and academic institutions also have a concern in this and related matters. Such practices threaten the basic relationship of trust among professionals. Cooperative endeavor and exchange among colleagues cannot flourish in an atmosphere of suspicion and distrust. Doubts and accusations flourish instead, all the more deadly be-

cause they cannot or may not be substantiated. They can only blight or destroy scholarship. Furthermore, relationships among scholars, administrators, and sponsoring organizations are placed in question. Secrecy dissipates responsibility and draws an invisible line between the knowing and the ignorant in a joint enterprise. Where responsibility is cloaked in secrecy and unity is displaced by division, fear, hostility, and recrimination thrive.

These considerations, to my mind, are secondary to more serious concerns. Since I did not know of any CIA connection with the research, I assume that my respondents did not know. Whether or not their knowing would have made a difference in granting the interviews, this misrepresentation violated a cardinal principle guiding research procedure as I understand it. The respondents had a right to know and to grant or refuse an interview as they saw fit. Still more serious was the potential violation of the respondents' anonymity. The cases in my files are identified by number only. Where of necessity or convenience names appeared on early draft interviews, they were cut off or blotted out. However, a list of names identifying cases was necessary at the Society Center to collate the results of the separate studies of each case. I do not know if the CIA had access to these files or, if it did, how it might have used the information. Respondents seemed to speak quite freely and frankly of their activities prior to and during the revolution, and frequently described the activities of friends, relatives, co-workers and others still in Hungary. Some of the respondents or people they knew played sensitive roles in affairs in Hungary; they well may not have been so candid in the interviews if they had known of the CIA involvement. Furthermore, respondents sometimes discussed rather personal matters that might have proved at least awkward or embarrassing if known to others, particularly since many of the refugees planned to live in the vicinity, and there was a substantial and long-standing Hungarian community in the city of New Brunswick adjacent to the refugee quarters. Thus, some twenty years after the fact, I find that I unwittingly

tingly mislead my informants, possibly violated their anonymity, and in the process I may have placed them or others in varying degrees of potential jeopardy.

Conclusions

The lessons that can be learned from this kind of experience are relevant to the academician in a variety of situations. Precautionary measures need to be formulated that would give reasonable assurance that professional canons, or those endorsed by persons immediately concerned, are observed.

Academic research is funded largely by private or public grants. First, the researcher must find out who or what is the source of the funds. In the eagerness to obtain research funds, or in the naivete that hindsight dispells, the researcher may not confront or even consider this question. And even if it is seriously considered, there is no certainty in the answer. Assumptions are easily (even traditionally) made that appearance is reality and trust and understandings are mutual. It is regrettable that this is not always the case, and that the luxury of "gentleman's agreements" must be relinquished. However, if assurances by the grantor that are crucial to the integrity of the research were documented and drawn up in the research contract, known violations of them might be subject to suit in a court of law. Often in research contracts, more is demanded of the recipient regarding his or her intent than is stipulated by the grantor. It also is usual for the recipients to present their credentials and a detailed account of the project before and during the research. As much might well be demanded of the grantor. Since most grants to faculty are made through their universities or colleges, such measures could be established as general policy. Professional associations might also endorse similar policies. Giving "official" status to such measures would help to relieve individuals of the onus implied by formalizing what is thought to be understood.

Governing the use of human subjects in research presents many difficulties, particularly where joint research or team efforts are involved. Rules governing pro-

fessional ethics in this matter are seldom formally stated; they are usually generalized and "understood." But what "goes without saying" may not be sufficient. Contractual and documented assurances that researchers and sponsors are who and what they claim to be may inhibit misrepresentation to the subjects.

Protecting the anonymity of subjects is another difficult matter. Particularly when more than one person is involved in the research project, which is likely, formal procedures should be used to guarantee anonymity. A carefully considered plan could be formulated in written form as part of the research design. Other stipulations pertinent to the integrity of the research could also be included. I have served in various capacities as an evaluator of many research proposals, but seldom if ever have I noted any attention to these considerations. Nor have I or other committee members serving as evaluators raised any issue concerning such matters. They usually are ignored or taken for granted.

The U.S. Department of Health, Education, and Welfare has prescribed certain procedures concerning research with human beings, which require review of projects by university review boards. Many colleges and universities now require that all members of their institutions doing research on human subjects file a statement with such a board, and that a continuing annual review be made as long as the research is active. Such statements might well be included in all research proposals and receive as careful scrutiny as the technical aspects of the proposal. This would not only permit review of procedures, but would also place researchers and others involved on record. In turn, the researchers would clearly understand their responsibilities, and a substantive reference would be available for evaluation of violations of them.

The profession also has a responsibility in the training of sociologists. More attention should be given to the ethical problems of research in classes on research methods, particularly for graduate students. Most texts give very little attention to the subject, and with few exceptions, research monographs do not discuss it.

There is need for more attention to what the canons of research should be, the controversies concerning them, ways in which they might be enforced, and possible sources of their violation.

At a more general level, reasonable controls need to be placed on agencies that sponsor and conduct research. Deliberate deception and misrepresentation are legally proscribed and morally condemned in most civilized human relationships. Where they are not, only overriding concern or urgent necessity may permit them, and provisions are made to give some assurance that such is the case. None of these conditions obtained in my situation, and very likely many other academically affiliated persons find themselves in this same situation.

Those who do are likely to face frustration and inaction. Covert activity usually is revealed long after the event, when significant details are remote or deliberately withheld and those involved are dispersed or beyond reach. Little can be done at the individual level, where time and resources are limited, to find out who else is involved, how they feel about the matter, or what the consequences may have been for the subjects of the research. Since individuals often learn separately—and indirectly—of their involvement, a concerted, collective response by them is unlikely, if not impossible.

Professional and collegial associations need to play a major role in addressing this problem. More than the isolated individual, they have the resources to investigate and the power to act on their findings. The individual's letters to relevant Congressional committees go unanswered, if heeded at all. Concerted protest and considered proposals by national and regional associations cannot be turned aside so lightly. Nor should the educational value of such action be overlooked, since those in authority and the public at large may not understand nor easily recognize the harm that can be done by violations of canons of research.

Although the experience detailed here may not be repeated, there is no certainty that it will not. Furthermore, this is but one example of the many kinds of problems that face those engaged in research. The widespread press reporting of CIA and other governmental research activities can only result in public skepticism and suspicion regarding research, particularly in the absence of adequate response from academic and professional sources. Failure to develop an understanding of the nature of scholarly activity and to protect the integrity of research may result in no human subjects for research and no resources to engage in it.

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RISK AND BENEFIT TO SUBJECTS OF FIELDWORK*

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During the past decade a complex network of federal regulations has been developed to protect human subjects of biomedical and behavioral research. These regulations contain an implicit model of the research process based upon medical experimentation. The technique of participant observation, as used in ethnographic research, differs in significant ways from this model. Consequently, researchers must begin to answer questions such as the following: What is the relationship between investigator and subject in participant observation? How does it differ from the investigator-subject relationship in experimental research? What is risk, and how and when does it occur in the ethnographic research process? What is benefit, and how is it related to risk? Until such questions are answered, regulations may be irrelevant and real ethical issues may be ignored.

Federal regulations to protect human subjects contain an implicit model of the research process. The definition of subject in terms of the relationship between subject and experimenter, of what risk is and when it occurs, and what potential benefits might be, are clear even when not clearly spelled out. The paradigm for such research is medical experimentation. Consequently, it fits smoothly upon some of the more formal and quantitative types of social research, especially psychological experimentation. It fits incongruously, however, upon that variety of research variously known as fieldwork, participant observation or ethnography.¹

To help determine the effectiveness of regulations based upon biomedical experimentation in protecting those exposed to fieldwork, I am going to compare various features of experimental and ethnographic research. First, I will explore some differences between an experimental subject and one studied by fieldwork, then I will investigate what types of risk people are exposed to in fieldwork and when these risks occur.

THE EXPERIMENTAL SUBJECT

Protective regulations use the term "subject" to refer to someone studied by various methods of research, ignoring the fact that the meanings of the term may alter depending on the research context. Thus, when we compare the subject of a traditional experiment to one studied by fieldwork, we find significant differences in power and control, direction of interaction, and scope of interaction.

Experimental subjects have relatively little power within the research situation: they are *subject* to the control of the investigator. Experimenters usually define and direct the situation in their own terms,

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¹ One finds great disagreement among social scientists on the content and objectives of human subject regulations. One reason for this is that the regulations are interpreted very differently by each institutional review board. Although medical review boards show variation (personal communication, Dr. Eric J. Cassell, Chairman, Committee on Human Rights, Cornell University Medical College), there is wider variation among review boards dealing with behavioral research. Thus, Dr. William C. Sturtevant, testifying before the National Commission for the Protection of Human Subjects of Biomedical and Behavioral Research, on behalf of the American Anthropological Association, noted that some prestigious private universities exempt ethnographic research from institutional review, while others merely give it a "boilerplate assurance of no risk." On the other hand, smaller, less prestigious institutions tend to apply the regulations with great literalness to fieldwork. The smaller institutions often have review boards consisting primarily of clinicians, experimenters and lawyers, who apply a restrictive concept of

risk, partially because of worry about DHEW blacklisting, and partially because of ignorance of what ethnographic research is all about (Anthropology Newsletter, 1977). When social scientists discuss human subject regulations, they are often actually discussing the particular interpretation of these regulations by the review boards at their own institutions.

with subjects having limited opportunities to question procedures (Kelman, 1972:991). The superior power and control of the investigator is particularly clear in medical experimentation, where subjects may be ill and need the doctors who wish to use them as experimental subjects. One definition of subject is someone *toward whom* action or influence is directed. This is true of the experimental situation: experimenters act; subjects (or their diseases or physiologies) react.

Subjects are not entirely without power. They can refuse to take part in an experiment, or they can leave. In short, they can reject the role of subject. But *as experimental subjects*, they have relinquished control over the research setting and situation.

Naturally, subjects may not respond exactly as experimenters wish: they may attempt to "read" investigators, reacting to subtle, unprogrammed features of the research situation (Orne, 1962; Orne and Scheibe, 1964; Orne and Evans, 1965). This type of interaction is outside the experimental paradigm. Because experimental research seeks to predict and measure responses, the human characteristics of subjects, which cause them to try to "read" or "please" experimenters, are irrelevant and distracting. No matter how humane experimenters and their treatment of subjects, their basic interest is in the process under study, with people conceptualized primarily as vehicles for, or carriers of, this process. In experimentation, every possible attempt is made to operationalize concepts, standardize measurements, isolate the effects of the setting, and reduce as far as possible the disturbing effects of observation upon that which is observed. Ideally, only those changes which occur as a result of experimental manipulation constitute the data of the experiment.

Symbolically, the limited and depersonalized relationship between experimenter and subject is expressed in the traditional terminology of psychological experiments, where the subject is an S, and the experimenter an E. The E manipulates the S, and the more parsimonious and elegant the experiment, the less the irrelevant human attributes of the S interfere

with the measurement of the variables being studied.

THE RELATIVE STATUS OF SUBJECTS

The differences in power and control between investigator and subject within the experimental research situation are compounded by the fact that much research is carried out on groups that are in some sense disadvantaged: patients, prisoners, addicts, military recruits (Kelman, 1972:991).

In fieldwork, too, participant observers have traditionally come from more powerful groups than the observed. Investigators from more affluent and powerful nations frequently carry out research in developing countries, or among deprived groups in their own countries; very rarely is the pattern reversed (Kelman, 1972:991; Beals, 1969:chap. 2). Hughes (1974:332) says:

Malinowski went out to the Trobriands and studied people whom he probably would not have liked to have to dinner. Radcliffe-Brown studied the Andaman Islanders who ate pig which they cooked on sticks over a fire. It is called anthropology, the study of anthropos, man. When finally we got around to studying the people here at home in our own cities we called that study "sociology," the study of our social companions. We are still studying people who are relatively deprived. We still keep in practice, with the idea that there are those with a mandate to study, and those with the fate of being studied whether to be preserved, as were the aborigines, or to be enlightened and rehabilitated.

Anthropology has been called "a child of Western imperialism"; there is the image of the ethnographer at the outposts of the empire, sitting on the verandah, sipping gin and bitters, asking the District Officer to send over a few "natives" to question. Whether this scene reflects myth or history, such a relationship is entirely untenable in today's "revolutionary and proto-revolutionary world" (Gough, 1968:405).

Although common, a status difference between observer and observed is not intrinsic to the fieldwork situation: ethnographers can study peers (Cassell, 1977,

1978); they can "study up" (Nader, 1974, 1976); or they can examine a large bureaucratic organization from top to bottom, focusing on the power and opportunity structures that differentiate leaders and the upwardly mobile from powerless individuals stuck in dead-end jobs (Kanter, 1977).

Naturally, when the participant observer comes from a group with greater power, prestige and material resources than the observed, the observer will gain easier access to individuals and information. But even in an extreme situation, the subject of ethnographic research has a certain ability to resist questioning, which a subject of biomedical experimentation does not have. Because of the open-ended and reciprocal nature of the relationship—the observer seeks information that must be volunteered, rather than comparatively passive compliance with a predetermined procedure—the observed has some latitude in blocking the curiosity of the ethnographer.² Those who are studied, then, are not entirely subject to the fieldworker; they have their share of

resources needed by the observer. Wax points out that in research among an exotic people, the ethnographer who wishes to learn the language and customs occupies a social position similar in some respects to that of a child. In addition, the investigator is often dependent upon the observed for basic needs, such as food, shelter, water and physical safety. Even in Western society, where those who are studied may seem to be politically subordinate, the fieldworker may have minimal power. He or she may not, for example, be able to live safely in ghetto housing without the protective hospitality of local residents (Wax, 1977:29).

When there is little or no status difference between observer and observed, entry and access to people and information are impeded. "Breaking and entering" is still more difficult when the ethnographer is "studying up." Daniels (1967:286; 1974), who investigated army psychiatrists and upper-class women volunteers, notes that for the intrusion to be tolerated, the lower-status observer must be of use to the observed. This usefulness may include playing the role of "jester" or "pet"—as revealed by the army officer who vouched for Daniels, once she was accepted: "'What? You don't know Arlene? . . . She's a great girl. She's our mascot. She studies us'" (1967:286).

² Witness the following conversation, reported by Evans-Prichard (1971:12–13):

I: Who are you?

Cuol: A man.

I: What is your name?

Cuol: You want to know my name?

I: Yes, you have come to visit me in my tent and I would like to know who you are.

Cuol: All right. I am Cuol. What is your name?

I: My name is Prichard.

Cuol: What is your father's name?

I: My father's name is also Prichard.

Cuol: No, that cannot be true. You cannot have the same name as your father.

I: It is the name of my lineage. What is the name of your lineage?

Cuol: Do you want to know the name of my lineage?

I: Yes.

Cuol: What will you do with it if I tell you? Will you take it to your country?

I: I don't want to do anything with it. I just want to know it since I am living at your camp.

Cuol: Oh well, we are Lou.

I: I did not ask the name of your tribe. I know that. I am asking you the name of your lineage.

Cuol: Why do you want to know the name of my lineage?

I: I don't want to know it.

Cuol: Then why do you ask me for it? Give me some tobacco.

THE FLOW OF INTERACTION

Unlike experiments, where the relationship between investigator and subject is primarily one-way, there is a two-way flow of interaction between ethnographer and those studied. Both observer and observed are using every available cue to read the other. Each has needs and ends; each interacts to try to meet needs and achieve ends. In point of fact, the observed are quite frequently more sophisticated than the participant observer in manipulating the other (see R.H. Wax's (1971:181–220) hilarious account of the experiences she and her husband had when living on a reservation with a family of "professional" Indians who specialized in bamboozling unwary outsiders).

Unlike experimental research, which seeks to predict and measure responses,

participant observation seeks to establish "understanding of the pattern of meaning and relationships" in the group being studied (Olesen, 1977:2). Instead of limiting the scope of interaction to control extraneous variation, the ethnographer carries on research amidst the "booming buzzing confusion" of daily life, with that very confusion being a part of the ethnographic data. Fieldwork is open-ended, a process occurring and altering through time as knowledge and relationships alter. In experimentation, instruments are frequently used to measure and record the changes in subjects (or their diseases) which occur as a result of manipulation; these changes constitute the experimental data. In fieldwork, change is more likely to occur in the observer than the observed; it is the ethnographer who alters as a result of interaction with those who are studied, and the responses of the ethnographer constitute part of the data. The fact that the fieldworker is his or her own measuring instrument makes it difficult to posit an absolute dichotomy between the mind of the observer and that which is observed. Malinowski discovered that "there is a radical incompatibility between the demands of scientific objectivity and the personal human involvement which participant observation necessarily entails" (Leach, 1974:2). Not only does the observing interfere with the observation, but *the interference itself is a significant datum*. Opposed to the objectivity, parsimony and control which is the ideal of experimental research, in ethnography we have perhaps not subjectivity but surely intersubjectivity, where much of the basic data come from an analysis of the interaction between the person who studies and those who are studied, in a situation where both "are at once observers to themselves and subjects to the other" (Devereux, 1967:275).

Human subject regulations assume a research situation where the more powerful experimenter controls the subject, with definition and direction of the situation proceeding from experimenter to subject. The fact that fieldwork differs in many respects from experimental research, however, does not mean that those who are studied need no protection. Human

subject regulations are designed to protect those who are studied from risk. And people are at risk in fieldwork. Because observer and observed influence each other in many ways, each may be exposed to "physical, psychological or social injury" (Code of Federal Regulations, 1977:46.103).

RISKS AND BENEFITS IN FIELDWORK

Risk and benefit in fieldwork occur at two different times, during interaction and when the data become public.

Interaction

1. *Risk*. During the interaction between participant observer and observed, various types of risk may be incurred. Deception involves risk: the observed may be at psychic or social risk if the observer is deceptive about his or her purposes. Smith (1976) points out that in cases involving deception, there is a conflict between the potential harm of the invasion of privacy, and the emerging value of the public's "right to know." There is no clear solution to this conflict through abstract analysis, nor is there a clear formula for the risk-benefit calculus. In each case, "complex human judgments are involved, ad hoc judgments guided by precedent and debate, in which movement toward consensus can be stimulated but hardly dictated by ethical analysis" (Smith, 1976:450). The literature contains few examples of long-term deceptive research, except for the case of the military (Sullivan et al., 1958), and most deceptive research has been possible only in situations where there was marginal or highly restricted interaction between observer and observed. The conspicuous case is that of Humphreys (1970), who observed illegal homosexual acts in public restrooms, deduced the identities of participants, and subsequently interviewed them in their homes under false pretenses. In thus penetrating their "fronts" as solid citizens, Humphreys gained the power to expose them publicly as persons who participate in deviant, stigmatized and illegal behavior. (See the critique by Warwick, 1973.) Deception threatens the ethnog-

rapher as well as those who are studied; it is the ethnographer whose ethical sensibilities are coarsened, occasionally to the extent that the validity of his or her work is jeopardized. Deception may also threaten the work in progress, since people may covertly recognize that they are being lied to and behave accordingly (Mead, 1969:374–379; Erikson, 1967).

Interaction may put people at risk in other ways. There is an emotional risk posed by the observer who takes pains to become part of a group's ongoing life and be defined as "friend" and on occasion advocate, and who then leaves, breaking off ties with those who were studied. What are an observer's obligations to the group when research is completed, and how long do these obligations last?

Risk may also be involved when the observer introduces a higher standard of living or a new technology to a group (Sharp, 1973). How much responsibility does the researcher bear for such a situation? How technologically "pure" should the group be kept? Should this "purity" extend to medical technology?

2. *Benefits.* Federal regulations for the protection of human subjects state that risk to subjects should be weighed against the benefits from proposed research (Code of Federal Regulations, 1977:46.102). Thus, in medical experimentation, when the risks are high, the possible benefits are also high. The risk from interaction during fieldwork is rarely as great as that from medical or other behavioral experimentation. But what are the benefits?

There may be material benefits to those studied: the observer can supply access to scarce goods, help, money. Abrahams (1970:8, 10) reports that young men in the Philadelphia ghetto, where he lived and collected folklore, borrowed his tape recorder to impress female friends and to rehearse rhythm-and-blues arrangements; Liebow (1967:243, 253) drove streetcorner friends to important appointments when they could not afford a taxi, and shared money and favors with them in the same way that any regular member of the group who had special resources was expected to share.

There are also intellectual benefits: the

satisfaction of being able to perceive more about his or her way of life and to analyze and discuss the interrelations among various areas can be of great interest and stimulation to an informant. "My best Sedang informant," reports Devereux, "once exclaimed 'I never realized that there were so many things in our culture!'" (1967:139). And Kanter (1977:296), when studying a modern corporation, had a small group of informants who told her about the history of the company, gave information about career issues, checked stories gathered elsewhere, and discussed with enjoyment concrete and philosophical issues relating to the new human problems of the corporation.

The fact that the investigator places high value on the information is an intellectual and emotional benefit: we all like to be found interesting. There are emotional benefits to incorporating a new person with a fresh viewpoint into a group, if only temporarily. Ethnographic informants have on occasion been described as deviants—this is supposed to explain why they waste time talking to the observer—but this "deviance" may be related to the fact that informants may be capable of a type of conversation and/or interaction which is rare in their own culture. Whyte's remarkable informant, Doc, who was a leader in his own group, moved from the position of key informant to that of research collaborator, discussing ideas and observations, helping to analyze the social dynamics of the community, and criticizing the manuscript of *Street Corner Society* (Whyte, 1955:301). As a frequently unemployed high school dropout, Doc must have found such intellectual stimulation rare and rewarding. With whom did Doc discuss sociological theories and ideas when Whyte returned to the university? (Naturally, this raises the question of whether the benefit of deeply satisfying interaction is worth the risk of losing it, but this problem is not unique to fieldwork.)

Access to Data

1. *Risk.* A more serious possibility of risk occurs after the research, when the data are made public or available to a par-

ticular audience, such as the sponsor of the research. Although the differences in power and status between the observer and observed are not intrinsic to fieldwork, there is a power differential which generally applies: the sponsor, when there is one, is more powerful than observer or observed, and neither has control over the sponsor's use of the data (Kelman, 1972). (This issue goes far beyond fieldwork, for the sponsor of survey or other social research is also powerful, and the sponsor's use of data is beyond the control of either the observer or those studied.) Counterinsurgency research in the 1960s, as represented by programs sponsored by the federal government (Horowitz, 1967; Beals, 1969; Deitchman, 1976), is a dramatic example. But there are less obvious cases of possible harm to subjects. Research on deviance or on disadvantaged groups can be used to control those who are studied, or to explain differences between them and the majority in terms of "social pathology" (Kelman, 1972:1009-1010; Rainwater and Pittman, 1967:302-303). Research which in effect "blames the victim" for a disadvantaged situation tends to generate policies designed to "revamp and revise the victim, never to change the surrounding circumstances" (Ryan, 1971:24). This can divert attention from attempts to eliminate barriers to economic opportunity and correct for unequal distribution of resources—policies which may be more effective in eliminating the effects of poverty than programs designed to alter the psychological characteristics of the poor (Kelman, 1972:1009-1010). There are those who believe that any exposure of cultural differences between minority and mainstream practices (or norms) may lead to "deficit" theories about the minority and to policies designed to alter these "pathological" practices. (The debate on the Moynihan Report deals with some of these issues: Moynihan, 1965; Rainwater and Yancey, 1967; Valentine, 1968.) Is it better, then, to ignore differences, because their exposure may lead to misguided or dangerous policies designed to eradicate them? Or is it wiser to explore these differences, indicating their function within the group being studied and their

relationship to that group's position within the wider society? (See Stack, 1974, for a thought-provoking ethnography which follows the second course.)

Related risks are posed by publication. As research data become public the ethnographer loses control over their use or misuse. Publication poses various levels of risk. These range from the characterization of a group in terms which many members may find unacceptable (Moynihan, 1965; Lewis, 1968), to the violation of anonymity, subjecting an individual or group to unwelcome publicity (Gallaher, 1964; Whyte, 1973), to exposing people to legal, institutional or governmental sanctions because of behavior revealed by the fieldworker (i.e., studies of illegal massage parlor sex-for-money in a comparatively easily-identified college town: Rasmussen and Kuhn, 1976). What is the responsibility of ethnographers in such cases? Should material be "cleared" with the group under study, and if so, with which members or factions within the group? How does one weigh the public's "right to know" against an individual's or group's "right to privacy"?

2. *Benefits.* The possibility of having data misused by a sponsor is balanced by the immediate benefit of having research funded. This is an uneven risk-benefit calculus: the researcher gains while those who are studied may be put at risk. When one person or group benefits from the risks of another, can risk and benefit be weighed against each other?³ A decision on whether the benefit to the researcher outweighs the risk to the research population must be made *before* the work is funded. This requires a careful selection of sponsors and some clear thinking about who is the "client" and exactly what is the responsibility of the investigator (Rainwater and Pittman, 1967).

Science benefits from the publication of research data, and this is a legitimate

³ May (1978) criticizes the risk-benefit calculus by noting that the terms are asymmetrical: "risk" implies the mere possibility or probability of harm, while "benefit" seems to describe virtually certain payoffs. He suggests that it might be more accurate to carry out risk-hope analyses or weigh harm against benefit.

benefit. The researcher benefits as well, since his or her career is advanced. The group studied may also benefit, not only at the time of publication but later as well. For example, sociological research has changed informed opinion about the normality of much so-called "deviant" behavior (Bell, 1960; Becker, 1963) and exploded many myths about the laziness and inarticulateness of the poor (Whyte, 1955; Lewis, 1961; Abrahams, 1970; Labov, 1972). A number of current American Indian land claims are based on earlier work by ethnographers studying entirely different problems (Lurie, 1977). There has been much talk of ethnographers "taking away" or "stealing" information, but occasionally the observer's work acts as the repository of material which may not be valued by the group until a later date (Devereux, 1967:249). Such preservation is a benefit to science, to society and to the observed group.

SUMMARY AND CONCLUSIONS

I have pointed out that the relationship between observer and observed in fieldwork is reciprocal and not necessarily hierarchical. In addition, risk can occur at two points: during the interaction, and when the data become public. How do these differences between participant observation and the biomedical model affect the results of human subject protective regulations?

Human subject regulations are designed primarily to guard subjects from risks resulting from the interaction between subject and experimenter. Despite the fact that it is recognized that risks can be delayed—a prime example being diethylstilbesterol causing vaginal cancer in the daughters of women who had received it during pregnancy—it is difficult to see how such long-delayed and unexpected risks can be guarded against. Human subject regulations concentrate primarily on the initial review of the experimental protocol. Although there are provisions for continuing review, such review occurs only during the life of the research project (personal communication, Dr. Robert C. Backus, Office for Protection from Research Risks, DHEW). In addition, com-

mentators note that "this continuing review sometimes becomes perfunctory" (Hershey and Miller, 1976:71; see also Gray, 1975:15–16).

Because of the hierarchical nature of the subject-experimenter relationship, protection involves some attempt to equalize the power differential. The asymmetry cannot be corrected, but subjects can be protected by telling them what may happen and allowing them to decline to enter this unequal relationship or to leave it at any time. The entire question of informed consent raises practical and philosophical problems. Smith (1976: 447–448) points out that it is difficult for anthropologists to explain to "natives" what they are really up to, no matter how hard they try. He also questions the meaning of "informed" when researchers are explaining across cultural gulfs. And Gray (1975:128) found that, despite signed consent forms, 39% of the subjects in a medical experiment performed at a large American university medical center did not know they were taking part in research. An additional 8% felt coerced and would have preferred not to participate.

The concept of informed consent and regulations designed to keep experimenters from misusing their power over subjects are somewhat tangential to the reciprocal relation between participant observers and observed. In a reciprocal relationship, where action and influence flow in both directions, it is impossible to predict the course of interaction and consequently to obtain before-the-fact consent from participants about what will go on during interaction. Even the topic or location of an investigation may change during fieldwork (Powdermaker, 1966). Wax (1977:323) points out that "under these circumstances many, if not most fieldworkers find it difficult to conduct research with their initial and prior conceptualizations, and they cannot reliably request approval for a project which may not prove to be the one actually conducted."

The comparative irrelevance of protective regulations based on a biomedical model to the most serious ethical problems facing fieldworkers may have been obscured by advocates of "scientific"

sociology or anthropology. The terminology of these behavioral scientists reflects their desire that the research situation resemble that of natural science. Thus Vidich, Bensman and Stein (1964:x) note that "statistical methods, computer technology, model building, cross-cultural uniformities, empirical replications, linguistic constructs, theory construction, and so forth, are terms whose scientific tone arises from linguistic habit."

Can regulations that conceptualize the research enterprise in a way that does not fit a particular type of research protect those who are studied by this technique? How do such regulations affect fieldworkers? Are the regulations ignored? Do investigators falsify responses and permissions when unable to obtain genuine ones?⁴ Or do researchers attempt to follow the spirit of the regulations even if the letter does not apply? An ethnographer might very well be tempted to ignore regulations that do not really apply, and this could extend to a denial that there are, in fact, any ethical problems at all associated with the research. Smith (1976:452) points out that administrative regulation of research ethics inevitably focuses on the extreme, unacceptable case, but that we might more properly be concerned with the ethical level of "normal" practice in our fields. (Let me emphasize that I am not arguing that fieldwork, because of its differences from biomedical research, does not need ethical regulation. I do believe, however, that inappropriate regulations may do more harm than good.)

It is time to start a dialogue among ethnographers, a dialogue which might include ethicists as well as representatives of those frequently studied by fieldworkers, to discuss ethical problems associated with fieldwork. We must heighten the sensitivity of ethnographers and students to the difficulties they may encounter.⁵

When fieldworkers are conscious of ethical problems before these problems arise, then the ethical level of normative practice will improve, appropriate regulations can be devised, and the extreme cases, which generate publicity, distaste and overregulation, will become less common.

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⁴ I was told of a researcher who was required by a university review board to obtain consent signatures from an entire tribe of American Indians before an ongoing study could be resumed. Believing after some time with the group that they would refuse to sign any sort of governmental form, the ethnographer signed them with the names of the Indians and went back to live with the tribe.

⁵ With the support of a grant from the Program in

Ethics and Values in Science and Technology, National Science Foundation, to Washington University, St. Louis, and the Center for Policy Research, New York City, I shall be a participant in such a project, devoted to "Ethical Problems of Fieldwork" (Murray L. Wax, Principal Investigator). The Project staff welcomes the interest and cooperation of our colleagues and professional associations.

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ONE OF OUR CONTRIBUTORS WRITES

I recently agreed to be interviewed by a graduate student conducting dissertation research on a professional issue. The student came to my office and began taping the interview without asking permission. I pointed out that were this a Federally funded study she would have to obtain written consent prior to conducting the interview or taping it. Her reply was, "Well, I'm glad this is not a Federally funded study." As the interview progressed, I grew annoyed and angry by the types of questions being asked. Although she had stated that she had no hypotheses in the study, it was clear that she was operating from a deficit model. All the questions were from a negative perspective: was I lonely? was I isolated from my peers? what problems did I have? what were the disadvantages of my job? what conflicts did I have?, etc. In spite of the fact that I was very annoyed at being taped without my permission as well as by the questions and felt increasingly defensive and put down, I did not attempt to terminate the interview.

Afterwards I realized how difficult it was to cut off an interview while it is in process. It caused me to reflect on the coerciveness of the interview situation. If as an agency administrator I did not feel free to terminate an interview with a graduate student, it must be almost impossible for the typical subject being interviewed by a "social scientist" to do so when the perceived status differences are reversed. I have discussed this incident with a number of colleagues who are in positions where they examine research proposals for ethical issues. We have agreed that it would take a very assertive person to stop an interview once it has been initiated. The issue has served to increase our sensitivity to the need for even more careful examination of what "consent" means. I hope the exchange of papers in *The American Sociologist* will serve to increase researchers' sensitivity not only to the need for consent, but also to the coerciveness inherent in the interview situation.

CONFIDENTIALITY AND THE PROTECTION OF HUMAN SUBJECTS IN SOCIAL SCIENCE RESEARCH: A REPORT ON RECENT DEVELOPMENTS*

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This article considers the legal status of promises of confidentiality by reviewing the work of the Privacy Protection Study Commission, the National Commission for the Protection of Human Subjects of Biomedical and Behavioral Research, and testimony presented before both Commissions by the American Sociological Association. Although the concerns of the two Commissions generally have not been seen as convergent, the protection of the confidentiality of research data is equally important in fulfilling the mandates of both Commissions in the case of social science research. Legal statutes granting immunity from subpoena of data collected for research and statistical purposes are the only means of guaranteeing confidentiality. Some of the problems of shaping a protective statute are discussed.

The maintenance of confidentiality is central to the protection of persons who participate as "subjects" in social science research. In the early part of 1977, representatives of the American Sociological Association testified before two federal Commissions—the Privacy Protection Study Commission and the Commission for the Protection of Human Subjects of Biomedical and Behavioral Research. The two Commissions are independent entities created by Congress to recommend guidelines and mechanisms for protecting persons from whom information is collected. This article reports on the work of the Privacy Commission and the Commission for the Protection of Human Subjects, the ASA's testimony before the two Commissions, and the legal status of promises of confidentiality.

Although the Commissions were created with different objectives in mind, their missions overlap in the case of social science research that must ensure confidentiality in order to protect its human subjects. The Privacy Protection Study Commission was created in response to the growing threat to individual privacy emanating from increasing data collection and record-keeping on individuals by various institutions. Naturally it viewed confidentiality as a primary concern. Indeed, in the Commission's early deliberations,

the protection of confidentiality outweighed the recognized need of society to collect and use information about individuals. The Privacy Commission's ultimate task was to fashion recommendations that would balance the protection of privacy and society's legitimate need for information.

The confidentiality issue is less central to the concerns of the Commission for the Protection of Human Subjects, but no less important when the risks of participation in social science research are considered. While in other types of inquiry—biomedical and some types of behavioral research—the immediate concern may be to find ways to protect participants' physical safety, the greatest risk of participation in social research is that information about identifiable persons will not be held in confidence by the researcher after the data are collected. The extent of possible harm to a research participant resulting from the disclosure of information could range from embarrassment to an adverse administrative action or, in some cases, even criminal prosecution. In designing procedures to protect the subjects of social science research, the Commission must also consider ways of protecting individual privacy.

THE PRIVACY PROTECTION STUDY COMMISSION

The Privacy Protection Study Commission was created by Congress in 1975 to

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study a variety of issues concerning the protection of American citizens' rights to privacy, and to examine the implementation of the Privacy Act of 1974 (5 USC 552 (a)). Among the issues the Commission was mandated to study were the rights of the recipients of public assistance and social services; privacy in the banking and credit industries; the protection of employment, personnel, and educational records; tax return confidentiality; mailing lists; insurance records; and the confidentiality of data collected for research and statistical purposes. Thus, although the confidentiality of data collected for social research may be a primary concern to the social scientist, the Privacy Commission had to consider the issue as one of many.

The Privacy Commission Hearings

The Privacy Commission published preliminary recommendations for protecting the confidentiality of research and statistical data in December 1976, several months before its final recommendations were presented to Congress and the President. In its preliminary recommendations, the Commission set the stage for the protection of the privacy of individuals about whom information is collected for research and statistical purposes with a broad statement: "As a general rule, any information or record collected or maintained in individually identifiable form for a research or statistical purpose should not be used to make any determination about an individual without the specific authorization of the individual to whom the record pertains" (Privacy Protection Study Commission, 1976:7). However, the rest of the preliminary recommendations belied the intent of this "general rule." For example, the Commission recommended that Congress provide by statute that no information about an individual collected for research or statistical purposes by federal authority or with federal funds be used for any other purpose without the individual's permission, *except* "as specifically authorized by Federal statute or as required by a court of competent jurisdiction" (Privacy Protection Study Commission, 1976:9). Also, in its recommendation concerning audits by

federal agencies of federal or federally funded programs, the Commission recommended that the information not be used to make a determination about an individual without his or her permission or "as required by a court of competent jurisdiction" (Privacy Protection Study Commission, 1976:10). The Commission also suggested that any records or other information not originally collected for research or statistical purposes may be used for research purposes. Subsequent redisclosure by the researcher would not be allowed without the authorization of the individual to whom the information pertains except for another research purpose or "as required by a court of competent jurisdiction" (Privacy Protection Study Commission, 1976:20).

Clearly, the Privacy Commission's preliminary recommendations supported the status quo regarding confidentiality by not suggesting that data collected for research or statistical purposes should be protected from subpoena and subsequent proceedings. The Commission recommended no protection from use in legal proceedings of information collected by a researcher. According to the preliminary recommendations, research data, like the information collected by journalists, should remain subject to subpoena.

The ASA responded to the Commission's preliminary recommendations at hearings held early in January 1977, speaking strongly against the use by legal authorities of information originally collected for research purposes. The testimony argued for the protection of research data by statute, stating that, "Unless there is statutory immunity, there is no real assurance that an agency can guarantee the confidentiality which it promises and with which it seeks to cloak the transfers [of data] which are discussed. . . . [I]t is important . . . to provide reasonable protection against court required disclosures" (American Sociological Association, 1977a:5).

The Subpoena of Research Data

Most social scientists have not had any experience with requests by legal authorities for data which they have col-

lected for research purposes. In fact, many social researchers probably have not been aware that a legal claim can be made on "their" data. However, lest they think the possibility of subpoena is so remote as not to warrant much concern, James Carroll and Charles Knerr have collected histories of several cases in which research data have been requested for purposes which would allow "a determination about an individual."¹ Carroll and Knerr (1977a:5-8) brought the following cases to the attention of the Privacy Protection Study Commission on behalf of the Consortium of Social Science Associations:

In 1972, a Harvard political scientist was imprisoned for refusing to reveal the identity of research resources to a Federal grand jury investigating the publishing of the Pentagon Papers. The scholar was released after eight days of imprisonment. The scholar contended that revealing information collected under assurances of confidentiality would diminish the potential for such research in the future.

In 1972, a sociologist reported the findings of a locally sponsored study of crime victimization, concluding that only a small percentage of victims report the commission of crimes to police authorities. The Police Chief of the city in question publicly disputed the findings and demanded the identities of subjects. The researcher could not provide the information since subjects names had not been kept, but stated that the identities would not be provided if they were available, since anonymity had been promised.

In 1972, an economist completed a Department of Labor sponsored study of teenage ghetto employment patterns. In the final report the researcher noted that a sizeable percentage of ghetto youth supported themselves through petty-criminal activities. The Board of Police Commissioners of the city in question subpoenaed the scholar and his research assistant, and demanded the iden-

ties of the youths who were interviewed. The economist refused to provide the information contending that absolute assurances of anonymity had been given the subjects. Pressures ceased shortly thereafter.

In 1971, researchers conducting a federally funded social welfare experiment in New Jersey were subpoenaed on fourteen occasions by a local prosecutor to reveal the identities of research subjects suspected of welfare fraud. General Accounting Office investigators and a U. S. Senate investigating committee also exerted pressure. The researcher refused to provide any data on the grounds that assurances of anonymity had been given research participants.

In 1974, researchers conducting a federally funded experiment involving ex-felons were pressured by local police officials to reveal the files of a subject suspected in a crime. The researchers refused to turn over the files, contending that anonymity had been promised.

In 1968, a sociologist conducting an evaluation study of a grant to a teenage gang in Chicago was subpoenaed on two occasions to turn over all research files to a Congressional committee investigating the Office of Economic Opportunity. The files were provided as demanded and the research project terminated.

In 1973, a researcher under federal grant to study selected business practices was pressured by two federal agencies to reveal the names of the businesses studied. The researcher hid the data in a shoebox in his home and threatened to go to jail rather than reveal raw data. The pressure ceased.

In 1973 and 1974, a sociologist conducting a longitudinal study of attitudes toward compulsory busing was pressured by a variety of actors—the mayor of the city involved, the governor of the state, the local federal judge, school officials, and others—to reveal research data and subject identities. In addition, the research was ordered terminated by a university research review board. The researcher retained an attorney and fended off all parties of interest and continued the data collection.

Conclusions of the Privacy Commission

The Privacy Commission issued its final recommendations for protecting the privacy of individuals in all the areas under its mandate, including research and statistical activities, in July 1977. Apparently, the testimony of Carroll and Knerr, the ASA, and many others representing the

¹ With a grant from the Russell Sage Foundation to the American Political Science Association and general sponsorship from the Consortium of Social Science Associations, Carroll and Knerr (1977b) conducted a major investigation of the status of promises of confidentiality, including a survey of social scientists, case histories of requests for data by legal authorities, and a listing of federal and state protective statutes. Their final report includes a recommended ethical statement for endorsement by social scientists.

research community influenced the final recommendations. The Commission recommended that Congress enact a statute providing that *no* information collected for a research or statistical purpose by a federal agency or with federal funds may be used to make any decision or take any action directly affecting the individual to whom the information pertains, except within the context of the research plan or protocol or with the specific authorization of the individual. In other words, data collected for research could not be used against a research subject. The Commission noted that it "is particularly important that the prospective data subject know of, and know that he can rely on, the limitations on use and disclosure as a basis for consenting to participate in any research project. It is equally important that users have no doubt about what uses are permitted and what disclosures they may make" (Privacy Protection Study Commission, 1977a:576). It concluded that, "The use of individually identifiable research and statistical records for administrative, regulatory, or law enforcement purposes encourages abuse of the expectation that information will be kept confidential" (Privacy Protection Study Commission, 1977a:568). For the present, however, research data on identifiable individuals are subject to subpoena and can be used as evidence in court proceedings or administrative hearings that may adversely affect individual research subjects.

Conceptual Problems in Shaping a Protective Statute

Recognition of the need for a protective statute is not a new idea. Some researchers have already arrived at this conclusion (e.g., Nejelski, 1976 and National Academy of Sciences, 1975), and there are now several statutes on the books. For example, the Secretary of the Department of Health, Education, and Welfare (DHEW) may authorize persons engaged in research on "mental health" to withhold the names of individual subjects of such research (42 USC 242(a)). Two factors limit the use of this statute: researchers must make special application

for the privilege, and the protection extends only to *names* of subjects and not to information collected about them. Also upon special request, the Attorney General may protect drug researchers from identifying their subjects. Grantees and contractors of the Law Enforcement Assistance Administration automatically work under the agency's protective statute, which immunizes any research data from legal processes without the consent of the research participant(s) to whom the information refers (21 USC 872(c)). Statutes also protect employees of the Census Bureau and of the Social Security Administration from required disclosure of the data those agencies collect. In addition, many states have enacted statutes which protect certain types of research data in limited areas (see Carroll and Knerr, 1977b).

None of the existing statutes applies to *all* of social research. All are limited in coverage by subject matter or sponsorship. These limitations indicate the problems inherent in actually drafting protective statutes. Defining "research" and "researcher" for the purposes of legal protection raises many difficult questions: What distinguishes data collection for research purposes from other types of inquiry such as investigative journalism or administrative audits? Should research be defined by the credentials of the researcher? By the nature of the methodology employed? By the affiliation of the researcher? By sponsorship of the research project? By the intended use of the data or the purpose of the research project?

These questions have been answered in different ways. Freidson (1976), for instance, sees two alternatives in defining social research. The first is to define research by method of data collection and analysis. While this alternative avoids the problem of defining the researcher by his or her credentials or affiliation, it would remain for some arbiter(s) (Freidson refers to them as a "credentialed elite") to decide what defines research methodologically and what does not. By this definition, which would rely on generally accepted standards, independent and unconventional researchers might suffer.

The other criterion for defining social research, according to Freidson, is the nature of the relationship of trust between the researcher and the research subject. With open research strategies, the relationship can be established legally by evidence in the research plan which proposes an open approach, along with promises of confidentiality and provisions for protecting the identity of research participants. In concealed research strategies, the researcher is not trusted as a researcher, but as a person. In these instances, Freidson suggests that "research warranting legal protection can be defined by the deliberate efforts of the researcher to protect the identity of his subjects even though he has not struck a bargain with them based upon an assurance" (Freidson, 1976:135). Such "deliberate efforts" would include a prior research plan, continuous systematic data collection, and recording and storage of data in ways designed to conceal the identities of the research participants.

Nejelski and Peyser (National Academy of Sciences, 1975) advocate a different approach to testimonial privilege for researchers. Their proposed researcher's shield employs two criteria for defining a researcher: (1) the individual must employ standards or principles accepted in his or her field of inquiry (the criterion opposed by Freidson) and (2) the information must be gathered for the "purpose of serving the general public in some foreseeable way." They argue that this definition does not require an implied or expressed promise of confidentiality. There is always the expectation on the part of subjects that information will not be released, and there are some methodologies (such as direct observation of the subject, description of the subject by informants and the use of secondary data) which do not provide ideal contexts for promises of confidentiality.

The Privacy Protection Study Commission attempts to solve the problem of defining "research" and "researcher" by relying on the sponsorship of the research project (the Commission's proposals are limited to government conducted and sponsored research), by addressing research data rather than the individual researcher, and by considering the uses to

which the data will be put, rather than by ruling on the methods of data collection. Thus, the Commission's recommendations for protecting confidentiality apply to information used for research or statistical purposes. The Commission uses the term "research" to refer to "any systematic, objective process designed to obtain new knowledge regardless of whether it is "pure" (aimed at deriving general principles) or "applied" (aimed at solving a specific problem or at determining policy). *Statistics* refers both to data obtained through enumeration and measurement and to the use of mathematical methods for dealing with data so obtained" (Privacy Protection Study Commission, 1977a:570). In defining the purpose of research and statistical activities, the Commission emphasizes "that research and statistical activities are undertaken not in the investigative sense of what there is to know about identified individuals, but in pursuit of systematic knowledge about human beings in groups" (Privacy Protection Study Commission, 1977a:571). (Chapter 15 of the Commission's report is devoted to recommendations for federal research and statistical activities. The recommendations are well summarized in articles in the November and December 1977 and January 1978 issues of *ASA Footnotes*.)

The Commission has suggested that its final recommendations regarding research and statistical data be implemented through a revision of the Privacy Act of 1974.² The suggested revision provides that "no agency shall use or disclose a research or statistical record, or any portion thereof, in individually identifiable form without the authorization of the individual to whom the record pertains . . ." except as provided by the Freedom of Information Act, to the Bureau of the Census, for another research or statistical purpose or unless the disclosure would "forestall continuing or imminent physical injury to an individual" (provided the information disclosed is limited to that situa-

² See the ASA Conference Report, *Conditions of Research: Proceedings of a Conference on Implications for Research of Selected Federal Regulations*, for a summary of the Privacy Act and other federal regulations.

tion) (Privacy Protection Study Commission, 1977b:165–166). The revision of the Privacy Act would also allow a research or statistical record to be released in compliance with a subpoena to assist inquiry into an alleged violation of law by a *researcher* or an *institution* or agency maintaining research or statistical records. If information is to be released in individually identifiable form, the individual must be informed beforehand and may contest the necessity of the release of the record. The Commission would also allow disclosure of records to the National Archives (which cannot release data in individually identifiable form). In any of these instances, the information could not be used against the individual research participant.

The statute proposed by the Privacy Commission is limited in coverage to government conducted and sponsored research. It would protect only information collected from individuals, and does not address the problem of disclosure of information pertaining to collectivities, which are often the focus of sociological and economic research. Even with these limitations, it is not certain whether Congress will be receptive to the Privacy Commission's suggestions.

THE COMMISSION FOR THE PROTECTION OF HUMAN SUBJECTS OF BIOMEDICAL AND BEHAVIORAL RESEARCH

The National Commission for the Protection of Human Subjects of Biomedical and Behavioral Research was mandated by the National Research Act (42 USC 2891-1) to make recommendations to the Secretary of Health, Education, and Welfare concerning guidelines for the protection of human research subjects. Like the Privacy Commission, the Commission for the Protection of Human Subjects has been concerned with a large number of issues which go beyond the ethical concerns of social science research alone: general ethical principles in the conduct of biomedical and behavioral research; the boundaries between medical research and medical practice; the nature and definition of informed consent; the disclosure of re-

search information; the application of research guidelines to the delivery of health services by DHEW; compensation for injured research subjects; guidelines for research involving children, the institutionally mentally infirm, and prisoners; fetal research; psychosurgery; research volunteerism; and the functioning of Institutional Review Boards. Confidentiality of data collected for social research is one of the many issues considered by the Commission for the Protection of Human Subjects.

Human Subjects Commission Hearings

In May 1977, the ASA presented testimony to the Commission for the Protection of Human Subjects at one of three hearings devoted to the topic of the functioning of Institutional Review Boards. According to the National Research Act of 1974 (5 USC 331), any institution that applies for funds from DHEW must have established an Institutional Review Board "to review [all] biomedical and behavioral research involving human subjects conducted at or sponsored by" the institution. The Boards review research proposals according to the regulations issued by DHEW (Part 46 of Title 45 of the Code of Federal Regulations). The ASA testimony, like most of the statements presented to the Commission, reacted to the shortcomings of the DHEW regulations.

The regulations state that the review is to consider whether the human research subjects are at risk. If it is decided that risk is involved, the review must further determine whether the risks are outweighed by any benefit that might come to the subject or by the importance of the knowledge to be gained, that the rights and welfare of the subjects will be adequately protected, that legally effective informed consent will be obtained, and that the research will be reviewed at timely intervals.

According to the regulations, a subject at risk is any individual exposed "to the possibility of injury, including physical, psychological or social injury as a consequence of participation in any research. . . ." "Legally effective informed consent"

means the knowing consent of the individual (or his legally authorized representative) who is in a position to exercise free choice and who is not subjected to any element of force or deceit. Each subject should be given an explanation of the procedures to be followed, a description of any risks to be expected, a description of any benefits, an offer to answer any questions, and an instruction that he or she is free to discontinue participation at any time. With very limited exceptions, legally effective informed consent must be documented by a written form signed by the research subject.

The risks of participation in sociological research and the requirement that written informed consent be obtained from research subjects were topics addressed in the ASA testimony. According to the testimony, sociological research rarely, if ever, involves physical risks to the subject. Psychological injury may result from participation in some studies—some lines of questioning may cause a subject to be embarrassed or emotionally upset. However, the greatest risk of participation in social research is that information about the respondent will not be held in confidence by the researcher. The risk of “social injury” in social research primarily involves the public identification of the respondent or the disclosure of information which may damage the status of the research participant.

Subpoena Power and the Protection of Human Subjects

The prevention of such a “social injury” resulting from the disclosure of information collected for purposes of research rests on the researcher’s ability to hold the information he or she collects in confidence. As illustrated above, research data are still legitimately subject to subpoena; hence, the requirement that written informed consent be obtained whenever it is determined that the risk of injury involved may prove detrimental to the research subject. On the issue of informed consent, the ASA testimony made one argument, namely, that the signed forms actually serve to protect the researcher and the institution rather than the

research subject (i.e., the form serves as a release). An additional point is that the consent forms could actually work against the research participant. In practice, the researcher retains the signed forms. In many one-time surveys (and for the most part, although we decry the limitations of such “snapshot” approaches to social life, this is what social scientists do), the signed consent form may provide the only evidence that a subject participated in the research. The researcher often has no reason to retain the names of participants once the data for the study are collected. Both the research data and the consent forms could be subpoenaed. In studies of prostitution, alcohol and drug abuse, gambling, and criminal behavior of any kind, for example, a subpoena could be fateful for the research participant.

The Case for the Protection of Data by Statute

A policy of statutory immunity from subpoena for data collected for research purposes would go a long way toward protecting the privacy of research participants as well as reducing the risk of “social injury.” The Privacy Protection Study Commission was persuaded that immunity of research data is an important goal. It remains to be seen whether Congress will agree and what the shape of a protective statute will be.

The Commission for the Protection of Human Subjects has not considered the issue of confidentiality directly. Yet, the signature of the research subject can only serve to endanger many subjects as long as research data and materials may be subpoenaed. Without a statute protecting the confidentiality of research data, consent forms signed by research participants and held by the researcher should not be required.³ The signed forms do not serve

³ There are still other arguments against the requirement of signed consent forms. The requirement may hamper research that seeks to avoid a control effect, which can bias research findings, particularly field research (see Wax, 1977). Galliher (1973) argues that if all research requires the open written consent of participants, “the only secret information obtainable is from individuals or groups who are too ignorant and/or powerless to demand the necessary limita-

the intended function of protecting interviewees and other research participants in social research, and the participants do not view them as protecting their interests. The Commission has issued some of its final recommendations, but at the time this report is being written (January 1978), it has not concluded its deliberations on guidelines for Institutional Review Boards. *Preliminary* recommended regulations would relax some of the requirements, recognizing that in some types of research there is minimal risk involved and that documentation of consent may place an undue burden on the research while adding little protection to the research subject. Even if the preliminary recommendations are accepted, however, it will be some time before their effect is felt in research institutions.

CONCLUSION

At this point, government policy can make or break the promise of confidentiality. While social scientists are trained to respect and protect their respondents' privacy and often go to great lengths to do so, no promise made by a researcher can legally withstand the challenge of a subpoena from a court of competent jurisdiction (unless the researcher is willing to refuse and accept the consequences). The Privacy Commission has recommended to Congress that government collected or sponsored research data be protected by statute. If such a statute is enacted, it will not only protect some research data, but it will also serve as a model for additional protective statutes and regulations. While pressing for governmental action, social scientists should continue to educate

themselves and their students in the protection of research subjects and devise means of better protecting the data they collect.⁴

⁴ As the discussion of legal protection of data used for research purposes continues, several social scientists are exploring ways of eliminating or minimizing the possibility that research data on identifiable individuals will be or can be used for nonresearch purposes. The work of Boruch (1971, 1974) and Campbell et al. (1975) are outstanding examples.

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tions upon the researchers. Much more is known about the indiscretions and alleged pathologies of the ignorant, poor, and powerless than any other groups higher in the stratification system" (Galliher, 1973:96). In addition, research participants do not recognize that consent forms are designed to protect them, although many participants comply with the requirement. I have encountered the following responses to the form from subjects: "You should give me a signed form so that I could present it if the information you have given me is not correct"; in another case, a participant remarked: "I understand. You need my signature to prove you actually conducted the interview."

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CALL FOR SUBMISSIONS

A number of readers have made inquiries about the possibility of a special issue of *The American Sociologist* on matters relating to academic freedom, both in the United States and abroad, where sociologists and other social scientists are experiencing a variety of pressures. We are contemplating some sort of attention to this problem, either in the form of a special feature or issue, or in conjunction with our continuing but occasional features on social science abroad. We would be most happy to receive either suggestions about such a feature (e.g., specific cases, countries, institutions, etc.) or submitted papers for editorial consideration.

COMMENTS

Ethics and responsibility in social science research are frustrating to discuss in print or in person: the issues are complex and highly contextual¹ so that clear-cut rules-of-thumb are impossible to formulate; they are value-laden and controversial so that discussion often degenerates into preachment, accusation and defense; anecdotes and hypothetical cases are plentiful but documented instances are few and inadequate; vested interests are many including especially professional reputations, career building, research funding, lucrative consultantships, prestige, contacts, junketeering, etc. Nevertheless, ethical issues are vital to social scientists, to the people we study, those who fund us, and those who read us. Therefore, regardless of the pain, embarrassment and frustration they engender, they must be confronted. These three papers are stimulating contributions in this direction. My remarks are intended to provide supplementary perspectives and citations rather than specifically to assess the authors' arguments.

Stephenson's article is unusually timely, for in recent months numerous revelations of CIA infiltration into American campuses and academic research have hit the headlines as a result of the availability of recently declassified documents (Zacchimo and Scheer, 1978; Duhl, 1978). It has long seemed ironic to me that the very academics who have claimed infringement of their academic freedom when their research has been challenged on ethical grounds or when their sponsorship has been questioned on moral and practical grounds, have often proved to be the ones who have jeopardized research opportunities and hence academic freedom for the rest of us by their promiscuous projects, methods, solicitation of funding and inattention to other than their academic self-interest (Berreman, 1971a, 1971b; Wolfe, 1978).

Any social scientist knowledgeable about peoples in politically and militarily strategic parts of the world is sure to have been repeatedly solicited by representatives of agencies eager to use that knowledge for purposes which clearly compromise our scientific and humanistic integrity and our social responsibility, at the expense of those we study. My first exposure to such attempted seduction was in March 1963, at the hands of the Special Operations Research Office (SORO) of The American University. SORO was seeking information via Project PROSYMS, for the Department of the Army, on peoples and cultures of the Himalayan area for use in psychological operations "in the event of military action in the general area . . . involving the United

States and a hostile and aggressive communist force," but also for use by other governmental and nongovernmental departments and agencies. In return for information, it was suggested that I might receive money, employment, fringe benefits, and anonymity. I refused, but occasionally and with diminishing frequency, I have received similar offers and inducements, not only from SORO but from other furtive organizations and individuals hoping I might have a change of heart and join the ranks of hireling, gumshoe social scientists. The climax of this sort of adventurism in my research area came in 1968, with the ill-fated Himalayan Border Countries Research Project of the University of California, which nearly destroyed research opportunities in the region and had repercussions throughout India and beyond (Berreman, 1969). In anthropology as a discipline, the nadir was reached with the revelations and years-long controversy regarding clandestine and military research in Thailand and adjacent Southeast Asia in conjunction with and support of American interests in the Vietnam war in the late 1960s. The American Anthropological Association (AAA) was riven as never before or since by the controversy which raged as a result (cf. Berreman, 1971c, 1973; Wolf and Jorgensen, 1970, 1971). One positive outcome was establishment of the AAA's Committee on Ethics in 1968, and adoption of its hard-fought and well-won code of ethics in 1971 (American Anthropological Association, 1970; Berreman, 1973). That statement attends specifically to the ethics of relations with those studied, the public, the discipline, students, sponsors, one's own government, and host governments.

In 1972, a Committee on the Potentially Harmful Effects of Anthropological Research (COPHEAR) was established by the AAA. COPHEAR failed to make a substantial contribution during the year and a half of its existence, largely because of lack of interest and cooperation in its efforts to establish a data base of examples of actual harmful consequences of anthropological research. But its purposes seem to me worth noting. Just as the goal of Committees on the Protection of Human Subjects nationwide has been to encourage ethical research practice by requiring that subjects of research be aware of and consent to the harm they may incur by subjecting themselves to research, so the goal of COPHEAR was to encourage ethical research practice by requiring that the initiators of research be aware of and accountable for the harm they may impose by undertaking research. In short, I would say, *informed consent*

in research has to be balanced by *informed initiation, informed design, and informed planning* of research. Thus, the aim of COPHEAR was not legislation or regulation of research ethics, but enhanced awareness of and information on ethical problems in research: consciousness-raising and acceptance of responsibility and accountability among researchers. This was precisely consistent with the letter and the spirit of the AAA code of ethics, whose epilogue contains these words: "In the final analysis, anthropological research is a human undertaking, dependent upon choices for which the individual bears ethical as well as scientific responsibility. . . . This statement . . . is not designed to punish, but to provide guidelines which can minimize the occasions upon which there is need to forgive" (American Anthropological Association, 1970: 16).

In addition to sources cited above, the *Newsletter* of the AAA is a continuing, available and excellent source of documentation on these matters for it is a forum for letters, resolutions, proposals and other brief statements on professional issues, including ethics and responsibility. Weaver (1973) lists a number of primary and secondary sources on these topics in the chapters entitled "The Social Responsibility of the Anthropologist" (Ch. 1), and "Anthropology and the Third World" (Ch. 3). Two general works, so recent that they have not yet been widely circulated, are Barnes (1977), and Diener and Crandall (1978). Although written by a social anthropologist and by psychologists, both are aimed at a broad range of social science ethical issues. Bond's article on confidentiality also calls to mind a useful report by Nejelski and Lerman (1971) on social science research and the subpoena.

Cassell makes the important point that if inapplicable ethical regulations are imposed on ethnographers' research projects, it may lead researchers not only to ignore the regulations, but ultimately to deny that ethical problems exist at all in their research. It seems to me that ethical regulations are even more likely to be ignored when they are thought to conflict with research plans, funding, sponsorship, career ambitions and the like. This is precisely what happened in the case of those scholars who sought to pursue the Himalayan Border Countries Research Project with Department of Defense money (Berreman, 1969), and on a grand scale, in the response to the AAA Ethics Committee's inquiry into clandestine and counter-insurgency research in Southeast Asia (cf. Berreman, 1973:53-55; Wolf and Jorgensen, 1970, 1971).

Academic colonialism (Berreman, 1969),

both external and internal, is frequently justified by invoking academic freedom in defense of scientific license, just as mission-oriented research is often cloaked in the rhetoric of pure science. Thus, moral irresponsibility is called freedom, and escape from human accountability is sought in the smokescreen of scientific objectivity, in disregard of the fact that science is done by people and that people are neither value-free nor unaccountable to their fellows.

With the approaching end of political and academic colonialism, moral imperatives and self-interest in research are converging. If the former have seemed convincing to a few, the latter may convince the rest as people to increasingly refuse to subject themselves to social research that does not conform to high standards of morality, social responsibility and accountability.

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Each of these papers is a useful contribution to our understanding of the law, politics, and administration of knowledge. By "the law, politics, and administration of knowledge" I mean the exercise of authoritative judgment, influence, and skill to affect who gets to know what, when, where, how, and why, and who gets what, based upon the development, regulation, and use of knowledge. From an analytical perspective, these papers fit well into conceptual frameworks suggested by such terms as "post-industrial society," "knowledge society," and the like. From a professional perspective, each of the papers addresses issues that researchers should be more familiar with, and that professional societies should have greater capacity to address on a systematic basis than is presently the case.

From a legal perspective, the paper by Kathleen Bond addresses a constitutional question under the first amendment, which involves something of a paradox. The first amendment of the U.S. Constitution is designed in part to facilitate a free flow of information to the public. The paradox is this: in developing information through the tools of research or of journalism, it is often necessary to limit the availability of one type of information—the identities of sources—in order to develop and publish other kinds of information—namely, what sources are willing to reveal, but only under assurances of confidentiality. The problem is a conflict in values. When, and under what circumstances, does the societal interest in the free flow of information exceed the interests of

prosecutors and other authorities in securing information needed for the disposal of litigation and other forms of the public's business?

From a political perspective, the question is *who* is going to determine whether or in what circumstances research data can be developed and held in confidence.

The problem is illustrated by the case of Samuel Popkin, which is summarized in the Bond paper. Popkin refused to identify his sources of information concerning American involvement in Vietnam, although he was ordered to do so by a federal grand jury investigating the publication of the Pentagon papers. The federal courts ordered him to name his sources, and he was imprisoned for contempt of court for refusing. Popkin was protecting individuals who had provided him with information, under assurances that he would not reveal their identities. While the motives of the grand jury and the prosecutors were not entirely clear, they may have been punitive in character. If the identities of those who had provided Popkin with information could be determined, the individuals so identified could then be appropriately punished.

As the Bond paper makes clear, there are many important questions concerning the establishment of a privilege for researchers and their sources and subjects. To those interested in establishing some form of protection, the final position taken by the Privacy Commission is encouraging. Although the recommendation of the Privacy Commission on the subject is not perfect, it constitutes a considerable advance over the existing state of the law.

The paper by Joan Cassell addresses another dimension of the evolving legal situation. In American law, rules, regulations, and procedures addressed to one subject area are sometimes transposed and applied to another subject area, often by analogy and inferences from precedent. As Cassell quite appropriately observes, the complex network of regulations that has been developing over the last decade or so to protect human subjects of biomedical and behavioral research is based upon an implicit model of the research process involved in medical experimentation. For the last five years, I have observed the development of a tendency to apply regulations derived from medical experimentation to other forms of research, often in an inappropriate and confusing manner.

From a political and administrative perspective, the development analyzed by Cassell can be attributed in part to social science researchers. While the officers and staffs of the major social science research associations have been very alert to developments in the

regulation of social science research in the 1970s, many social science researchers seem indifferent to these developments. As part of the study that Charles Knerr and I conducted in the early 1970s with the cooperation of the major social science associations, we administered a survey of the experiences and attitudes of several hundred social science researchers concerning the confidentiality of research sources and data. The questionnaire was developed and administered in part by the National Opinion Research Center. While the following observation is what I will characterize as a speculative inference, to my mind the results indicated a lack of knowledge on the part of many social science researchers about the growing emphasis on the regulation of social science research by government. Researchers naturally resent comparisons of their research process with the activities of regulated industries, such as banking. This is particularly true of academic-based researchers. While this attitude may be understandable, research is increasingly becoming a "regulated industry." The Cassell paper is quite valuable in pointing out that the *kind* of regulation can make a difference.

To some extent, Richard Stephenson's paper suggests one of the reasons why research is becoming a "regulated industry." Regulations often become necessary to protect as well as to limit various actors in an enterprise. While regulations requiring grantors to be explicit about who is funding what research for what purposes might not have prevented what happened in the CIA cases, at least such regulations might have provided a means of redress for the wrong that was done.

As a sometime government official as well as an academic, I professionally oppose greater government regulation of research, particularly academic research. However, much research is enmeshed in value conflicts, which somehow must be resolved. I strongly endorse Stephenson's observation that professional and collegial associations have a high stake in these matters and should do more to understand what the issues are and how they might best be resolved. These papers and this symposium are a help.

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These three papers take up issues of ever greater ethical importance: the ethics involved

in field work, the ethics involved in maintaining confidentiality vis-a-vis research subjects, and the ethics involved when researchers are deceived by an undercover government agency.

The problems Cassell raises are perhaps not so much of political as of methodological interest; or rather, they are problems of human decency rather than professional ethics. Cassell believes that reciprocity should be achieved between the researcher and his or her subjects so that the latter will also derive some benefit. Whether this can be the case often depends on the status position of the researcher relative to that of the research subjects—the powerful don't have as much to gain from analyzing mechanisms of superordination as the powerless do from finding out what it is that maintains their underdog status.

Cassell makes a contribution to the study of the social structure of research. The researcher is not always of higher status than the subject, but it is a fact that it is easier to do research on subjects who are lower in status. This is not only because it is more "comfortable" not to have to fear arrogance from people who consider themselves superior, but also because the higher-status subject can more easily act as a gatekeeper and refuse access to data. It is easier to get prisoners to talk freely than to do research on prison administration. It is easier to interview nurses than physicians. But there is more: usually the problems themselves are being defined as problems of the underdog. Thus, when discussing the family, textbooks or agencies are likely to refer to the "problem child," not the "problem parent." And if they should realize that a parent is involved, it is the mother who becomes "a problem." And *if* the agency comes to realize that the father is part of the problem, it is the lower-class father. Upper-class fathers have rarely been the subjects of family research. The selection of research subjects, just as the definition of what is a social problem, is stratified. It is in this sense that we should understand Alvin Gouldner's (1968) appeal of some years ago to study those in power instead of the powerless.

Those who are low in the stratification system are also likely to be less educated, and this becomes an issue of some importance when the subjects' consent is sought. As Bond notes, subjects don't always understand what it is they are signing on "informed consent" forms. And if they know, they may be too intimidated to refuse their signature—not because the researcher intimidates, but simply because of the nature of the relationship. I conclude from Bond's paper that the informed consent of a research subject has meaning if the information

can be kept confidential, but not if it can be subpoenaed.

It is certainly heartwarming to know that most of the researchers who were subpoenaed in the last decade or so refused to reveal the identities of their research subjects in spite of threatened or actual imprisonment. Most graduate students do not know that at any time their data may be in danger of being subpoenaed. Statutory immunity for researchers would seem to be a *sine qua non* for research to go on unhampered. While such legislation is the first thing to be pressed for, it should be kept in mind that departments and universities would still have to face informal pressures from political and community agencies. College or university public relations offices surely are aware of this. A community agency can pressure the administration of a university to prevent some research, or to reveal information, and the administration in turn can pressure a department, which in turn can pressure the researcher. The issues raised by Bond ought to be taken up not only by professional associations and committees, but in every graduate department and in graduate research courses.

Political pressures upon research and researchers are bad enough, and the legal pressures of a subpoena are worse. But such pressures can be fought, even if it means going to jail. The case of undercover agencies commissioning research is qualitatively different because deception rather than pressure is involved. This deception introduces considerations of political, military or legal expedience, distrust and corruption into an endeavor that is socially defined as being based on mutual trust, and that must be open to the scrutiny of all. As Stephenson notes, these practices "threaten the basic relationship of trust among professionals" (131). In 1967 Lewis Coser wrote that "mutual trust is indispensable to any democratic polity. . . . If you can no longer deal with your fellows, with scholars or student leaders, with labor officials or educators, without suspecting that they express the hidden motives of their paymaster rather than their own self-willed intentions, then the public dialogue, without which democracy is inconceivable, is no longer possible." We might add that what is true for the public dialogue is all the more true for the dialogue between scientists and scholars. They cannot do their job without the right to know.

This sort of deceptive practice can bring most research to a halt, for people will refuse, and rightly so, to cooperate with a researcher if they suspect that behind the scholarly claim there lurks a secret political purpose. And re-

searchers will be difficult to recruit if they get the reputation of possibly being police agents. Efforts to obtain informed consent or to assure research subjects of confidentiality cannot withstand the assault of secret manipulation by any political agency, whether by government or other organization.

The question is not merely how to protect ourselves as researchers. The very norms of the scientific, scholarly and intellectual community are at stake. I agree with Stephenson that the issue should be a matter of concern not only to professional associations but also to graduate departments because this gives the opportunity to stress the value system of science and the professions. Finally, let me add that this is a matter for university administrations as well, as the Rutgers example illustrates. Not only should professional associations encourage graduate departments, faculty and students to discuss issues of agency infiltration; university departments in turn should press their administrations to scrutinize the sources of research funds and research assignments.

Possible subpoena of researchers and unwitting involvement in research sponsored by undercover agencies deserve the concerted attention of the scientific, scholarly and intellectual communities.

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Imagine for a moment that you are an ordinary human being, rather than a Professional Social Scientist Duly Accredited By Official Bureaucratic Procedures. Now read these three papers by Social Scientists Bond, Cassell and Stephenson. Then ask yourself these questions: What kind of society do these people live in? What exactly are they concerned with? Why are they concerned? What do they think they can do about it? What do you think they should do? Since we ordinary human beings recognize that all such answers depend on the circumstances (situations) we face at a given

time, we know there will be some disagreement. But I suspect you fellow ordinaries will see these essays in roughly the following way.

These authors live in a society of massive, convoluted, bureaucratized government powers. There appear to be millions of Bureaucrats who spend their entire lives "legislating morality," regulating minute details of speech and action, brandishing unknown powers that appear all the more immense for their unknown-ness, and continually launching new attacks from new angles. Are these Roman Bureaucrats acting under Caesar Augustus to "protect the rights of all subjects"? Are they Gogol's Bureaucrats counting dead souls? Or highly efficient Nazis "regulating" education to "protect the purity of the Aryan race"? Does it matter?

These authors are profoundly anxious, both about what the Bureaucrats *might* do to them and about some unknown (or merely unspeakable?) evil they themselves *may* already have committed. What strikes us ordinary people so forcefully is what does *not* concern them. We expect a scientist to be deeply concerned with getting at the truth of things and willing to face great risks to do so. We remember all those stories about Galileo insisting on looking through that telescope at those moons, in spite of the fact that he could not possibly calculate a cost-benefit schedule over the succeeding five centuries (or billion millenia), and in spite of the dangers of getting burned at the stake. Are today's Bureaucrats more ferocious than the Inquisition? Is that why today's scientists are so overwhelmingly concerned about their private and professional risks?

That paralyzing fear of the Bureaucrats is not the only thing that concerns scientists. Much of the time they seem most deeply anxious about what they and their colleagues will think of each other. They seem to have some immensely complex and sensitive "interactional moral radar" tuned to all other members of their society. Galileo might submit under threat of death, but he did so with great self-confidence and with defiance—"It still moves!" He knew that truth is invaluable to all human beings for all time. He also knew that the Church Bureaucrats were corrupt, immoral men of power who cared not a damn for all the sham morality they used to deceive the ordinary people into submission. The authors of these papers completely lack Galileo's self-confidence. They raise more questions in three little essays about the morality of their own purposes and actions than the Inquisition ever asked of all the Albigenian heretics. They seem to seek absolution in self-denial and self-flagellation. Even when they are absolutely

convinced they are the victims of the Bureaucrats (such as the CIA), they are profoundly anxious about "what people will think of me."

This deep self-doubt and, possibly, feeling of secret guilt has paralyzed the ability of scientists to protest against the Bureaucratic "regulators." We ordinary people react to attempts to regulate our free speech and right of assembly with outrage and fury—"Who the hell do they think they are trying to tell me with whom I can associate and what I can say and not say? Everyone knows those lousy Politicians and Bureaucrats are the biggest, most successful liars in the world! Who the hell are they to pretend to be more moral, humane and decent than us common people?" But these authors identify with their repressors. They never once assert that *they* are the moral and humane people and the Bureaucrats the evil ones. Even when one of them is obviously victimized by the total deceit of the CIA, he immediately calls for more Professional Bureaucratic Regulation! It never once occurs to any of them that decency and kindness cannot be legislated and bureaucratically created, but must be created and maintained by ordinary human individuals. Social scientists today do not speak of living in a concentration camp, yet they have built towering moral barbed wire fences around themselves so that they dare not even question the moral intent or effect of their repressors. Neither Kafka nor Djilas lived in a worse world than they are building for themselves.

And what should they do? It all seems so clear to an ordinary human being, certainly to the Homo Americanus type. What should an American do to anyone who tries to prevent the seeking and speaking of the truth? As an ordinary American human being, you know right well what should be done. Just forget all about being a Professional Social Scientist and you'll see immediately what to do. You, like the authors of these papers, are intelligent, honest, decent and moral people. Get on with your truth-seeking about human beings! Each bit of success will benefit all humankind through all the millenia. When the Imperial Bureaucrats try to repress your free speech "in order to protect the people," act like any ordinary American would. In case your years of Professional Education have dulled your memory of what that means, let me suggest two steps. First, tell them—"Nuts!" If that doesn't stop them—Fight like hell!

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The authors of these papers raise a number of important and interesting questions about social research in the United States. All three deal in one way or another with two central questions—how research can take place without harassment or cooptation by agents of the state, and how it can avoid doing harm to those it studies. These two questions are interrelated, for our capacity to do independent research hinges on our ability to protect those we study. In order to protect the subjects of our research, we need either legal protection or policies of our own adoption which can accomplish the same end. I will address the matter of legal protection first.

The Constitution of the United States is designed to limit the power of the state. The First Amendment establishes the legal foundation for the independence of media of information and opinion, predicated on the idea that citizens must have sources of information and opinion that are separate from the state. In other words, the First Amendment seeks to preclude a state monopoly over the collection and distribution of information.

This is the basic Constitutional ground on which the press stands, and the ground on which social research can stand. Like the press, social research can argue that its role should be an independent one, its function being informational rather than prosecutorial. Thus, it can be argued that research data should not be used by the state as evidence to prosecute individuals, groups and organizations. In view of the fact that, as Bond notes, state officials have sometimes attempted to coerce researchers into providing information that would incriminate some of those who cooperated with their studies, testimonial privilege could serve as a shield for the independence of research and the confidentiality of its data.

The chances for such testimonial privilege, however, are very slim. Save in a few states, not even the press has such privilege. Nor has the Supreme Court provided much encouragement for the idea since its *Branzburg* decision. As Bond notes, social research financed by certain federal agencies can be granted protection, but that is only for research acceptable to those agencies. And finally, if the pending bill S.1437, successor to the notorious S.1, is passed in its present form by Congress, the future capacity of both press and research to truly protect their sources from the state will be even feeble than it is now. Like it or not, social researchers are and will be in the foreseeable future pretty much on their own, without legal protection. They must take their

own actions to assure that they can maintain their independence.

The key to independence lies in the researcher's ability to protect the identity of his or her sources of data, for without such ability, access to many sources becomes difficult and the validity of data becomes problematic. In the absence of testimonial privilege, the only way of *absolutely* assuring the confidentiality of sources is to destroy all record of their identities. It might be argued that such a drastic step is unnecessary. Some laudably concerned researchers have invented highly sophisticated methods of protecting identities while preserving a record of them. But all such methods are predicated on the assumption of cooperation by the state and its judges. Without such cooperation, the researcher can be ordered to break the identity-code, have the names sent back from the foreign country in which they repose, or whatever, on pain of citation for contempt of court. Since there is no ground for presuming that the state will always be indifferent toward research data, I suggest that the only true protection, immune to shifts in the political winds and changes in state policy, lies in the routine destruction of identifiers the minute they are no longer necessary for the planned research.

So long as we protect our sources, we will have access to data not available to the state and so we can serve an independent research role. But we must face the fact that when we grant our sources the absolute protection created by destruction of identifiers, we do so at some cost to research itself. With identifiers destroyed, we cannot return to the same individuals for a new study at some later time. It is, however, very rare that such follow-ups are ever done. To take the risk of preserving identifiers for *all* studies on the off-chance that at some unforeseen time in the future one or two researchers might wish to do a *few* follow-ups is, to my mind, to balance only speculative benefits against very real risks.

All in all, we should forego our wishful daydreaming about how many interesting studies we *might* do were identifiers preserved in research and, for that matter, in administrative records. Instead, we should employ our intelligence to show how most if not all questions of both practical and theoretical significance can be answered by research which does *not* rely on the retrieval of identifiers in recorded data. And we should use our ingenuity to document how easy it is to breach—both covertly and by subpoena—any security arrangement claimed to protect recorded identities. By providing the arguments and evidence that discredit those who can see

no serious risk to both individual privacy and the independence of social research in the preservation of identifiers, we would go a long way toward establishing the basis of our right to ask for the trust of those we wish to study. What we would gain is far more important than what we might lose.

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The papers by Bond, Cassell, and Stephenson remind us that discussions of the ethics of various research practices are incomplete today if the proper role of government is not addressed—whether by noting areas where existing regulations are unrealistic or cause undue problems for research, by identifying needs for additional regulation, or by arguing that an issue is not appropriately addressed via regulation. Rather than comment directly on these papers, I will share some additional information about matters raised by Bond and Cassell.

The fact of governmental regulation of research involving human subjects is unlikely to change in the foreseeable future. The content of that regulation, however, is mutable. We are now approaching a period of flux in federal regulations for protecting human subjects, because of the recommendations of the National Commission for the Protection of Human Subjects¹ and the likelihood that the Department of Health, Education, and Welfare (DHEW) will respond by proposing revisions in its regulations for the protection of human subjects.² In this process there will be one or two formal opportunities for sociologists (along with other members of the "public") to make their views known.

Since 1966, DHEW has had requirements for institutional review of proposed research in-

volving human subjects. Also since 1966, the stated policy has been that those requirements applied to social research as well as to biomedical research. The review requirements have become more formalized over the years, changing from Public Health Service policy, to DHEW policy, to a matter of federal statute and DHEW regulations. They have also become more detailed and explicit, and the composition requirements for review committees have been broadened. In addition, the review requirement, which originally applied only to research funded by the Public Health Service, was extended by the National Research Act of 1974 to all research involving human subjects that is conducted at institutions that receive funds for such research under the Public Health Service Act. The Commission for the Protection of Human Subjects will probably recommend that the requirement be extended still further.

The National Commission for the Protection of Human Subjects of Biomedical and Behavioral Research was created by Congress in the National Research Act of 1974 and directed to advise DHEW and Congress about a number of complex issues of ethics and policy. Two points about the Commission's mandate are worthy of special note.

First, the Commission was not asked to proceed by determining whether subjects had been harmed in research and recommending ways to prevent future harms. Instead, the Commission was directed to begin by identifying a set of ethical principles and then to recommend ways to ensure that research is conducted in accordance with those principles. The focus on affirmation of principle rather than avoidance of harm should not be regarded as either trivial or accidental.

Today, most serious analyses of the ethics of research involving human subjects see the investigator's obligations to subjects in terms of respect for their personal integrity and autonomy, not only (or even primarily) in terms of avoidance of harm (cf. Ramsey, 1970; Veatch, 1976; Engelhardt, 1975). Some of the cases that have drawn public and governmental attention to the ethics of research involving human subjects were instances in which there was no evidence of harm to subjects. This was true, for example, in the scandal at the Jewish Chronic Disease Hospital in the early 1960s when researchers injected live cancer cells beneath the skin of non-consenting geriatric patients (Katz, 1972:9-65). Most critics of this research did not rest their objections on arguments that subjects had been physically harmed; objections rested instead on the per-

¹ This article, drafted in April 1978, contains several statements regarding recommendations by the Commission. These statements are based upon *draft* recommendations that have been approved by the Commission but not yet formally issued. The relevant reports are scheduled for release prior to the publication of this article.

² 45 *Code of Federal Regulations*, Part 46. Current DHEW regulations have been reprinted in a booklet available free from the Office for Protection from Research Risks, NIH, 9000 Rockville Pike, Bethesda, MD 20014.

ception that a moral harm had been committed, a transgression against the autonomy and integrity of the subjects, a violation of their rights. Criticisms of the ethics of some social and psychological research (e.g., research involving deception or disguised observation) often rest on similar grounds. From this perspective, the argument that few people have been harmed in research—which is true enough—is largely irrelevant. The purpose of the DHEW regulations, the review procedures, and the Commission goes beyond the prevention of harm to subjects, and researchers who act with disregard for the autonomy or dignity of their subjects are as likely (or more likely) to bring societal wrath onto research as are those whose research results in physical harm to subjects, particularly if the harm occurs in research in which there is good quality of informed consent.

The second noteworthy point about the commission's mandate is that, although the words "social research" appear nowhere in the legislation ("behavioral research" is left undefined) and there is little evidence that particular concern about social research had any role in the creation of the Commission, the Commission's fulfillment of its mandate will inevitably have implications for research in the social sciences. The Commission was directed "to identify the basic ethical principles that should underlie the conduct of research involving human subjects." It would be difficult for social scientists to convince anyone that the rubric "research involving human subjects" does not include much empirical work in the social sciences, although the term "subject" is not always used in such research. It would also be difficult to argue that social and biomedical research should be conducted according to different sets of ethical principles. (There may nevertheless be some relevant differences among research methodologies regarding, for example, the extent to which the procedures used in the research (a) are constitutionally protected and (b) would require informed consent whether or not they were being used for research purposes.) There are also clear implications for social research in the Commission's mandate to recommend ethical guidelines and administrative actions to implement them and, specifically, to make recommendations regarding institutional review boards (IRBs). A significant part of the research reviewed by IRBs is social research.

Social Researchers and the Commission

Social scientists who follow these matters were distressed when Secretary Weinberger's

appointments to the Commission were announced in late 1974, and no social scientists were included. The Act specified that no more than 5 of the 11 members of the Commission could be persons who had conducted research involving human subjects. Three physicians and two psychologists filled those slots. The Commission has been aware of this limitation in its composition, and has not been insensitive to issues brought to its attention by me, as a staff member, or by social researchers, who have been quite active in making their views known to the Commission both in correspondence and in hearings.

The Commission sought out the perspective of social scientists when undertaking sections of its mandate to which those views seemed relevant.³ It seems likely, however, that the same views expressed by a member of the Commission would have greater impact.

Survey of Human Subjects Review Committees

The Commission used the methods of social research to obtain information about the functioning of IRBs and the research that they review. This study was conducted for the Commission by the Survey Research Center at the University of Michigan, and involved a sample of 61 institutions. More than 2,000 investigators were interviewed about their research and their experiences with IRBs, and more than 800 review board members were interviewed (Cooke et al., 1977; Gray et al., 1977). Although this is not the place to describe the major findings of the study, some data bear on the topic at hand.

Research investigators were by and large supportive of the existing review process. Almost all said that the human subjects review procedure has protected the rights and welfare of human subjects, at least to some extent; two-thirds said that the review procedure has improved the quality of the research done at the institution; and, interestingly enough, almost all said that the procedure runs with reasonable efficiency. However, substantial

³ At its three hearings on institutional review procedures, for example, the Commission heard from sociologists John Clausen, Wallace Gingerich, Howard Higman, Ada Jacox, Hans Mauksch for the ASA, Virginia Oleson and Edward Rose. Among the sociologists who prepared papers for the Commission were Bernard Barber, on the assessment of risks and benefits in the review of proposed research; Donald Campbell, on issues in program evaluation and social experimentation; and Albert Reiss, on informed consent and confidentiality in social research.

minorities—ranging from one-quarter to nearly one-half—indicated that they felt that the review process is an unwarranted intrusion on the investigator's autonomy, that the IRB gets into areas that are not appropriate to its function, that it makes judgments that it is not qualified to make, and that it has impeded the process of research. But, fewer than 10% of the investigators felt that the difficulties of the review procedure outweigh its benefits in protecting human subjects.

However, on the various attitude measures about the review process, there was a consistent tendency for "behavioral researchers" (mostly psychologists and sociologists) to have less favorable attitudes than biomedical researchers. The data bearing on the explanation of this pattern have not been fully analyzed. However, it is notable that behavioral researchers were slightly less likely than biomedical researchers to have had their research proposals modified by IRBs. (Incidentally, very few proposals are actually turned down.) The most frequent modifications made by IRBs pertained to informed consent, a frequent topic of contention among social scientists who have complained about existing DHEW regulations. Yet, biomedical projects (27%) were more likely than behavioral projects (20%) to have undergone consent modifications at the hands of an IRB.

A number of explanations could be advanced to account for behavioral researchers' less favorable attitudes toward the review process. I will mention two possibilities.⁴

The first follows from the finding that little research is turned down by IRBs but that much is modified in some way. IRB modifications of social research may be more likely to have implications for the validity or methodological rigor of the study than modifications in biomedical research. As I noted, the most frequent modifications made by IRBs pertain to consent. In studies in which validity rests on subject naivety or on the adequacy of a sample, such modifications may have important implications for the soundness of the research. In biomedical research, more elaborate consent procedures may make it more difficult to recruit subjects, but will usually have no further methodological implications. Where nonresponse is a serious methodological concern (as opposed to a practical concern about the simple need for numbers), anything that increases

the nonresponse rate is likely to raise the researcher's concern about bias.

A second explanation posits a difference between biomedical and behavioral researchers in their orientation toward authority structures and in their awareness of the politics of research. For both historical and organizational reasons, the behavioral researcher may be more likely than the biomedical researcher to perceive the review process as inconsistent with the concept of freedom of inquiry. Hospitals and medical schools have authority structures that differ from the university; few in medical schools, for example, question the department chairperson's right to have significant influence over the research carried out in his or her department. That would be a major contention in a university. In addition, the content of social research is more often manifestly political than that of biomedical research; no one would want someone who is ideologically hostile to have control over one's research. (For a brief analysis of the dangers of politicalization of IRBs, see Gray, 1977.)

Recommendations of the Commission

Space permits only the briefest summary of the Commission recommendations that have implications for social research.⁵ Under these recommendations, IRBs would continue to play a central role in the protection of human subjects; the recommendations are directed at improving the performance and clarifying the responsibilities of IRBs. The Commission did not find it necessary or useful to issue separate recommendations for biomedical and behavioral research. It should be noted, however, that it concluded that IRB deliberations should take into account a wide variety of characteristics which distinguish research projects one from another.

The Commission defined "human subjects" (the lack of such a definition in present regulations has been a source of problems) as persons about whom an individual conducting scientific research obtains data through intervention or interaction with them or through access to

⁴ An explanation not examined here is that social researchers, like other scientists, are simply protective of their autonomy. On this point, see Barber (1975).

⁵ This summary is intended only to provide a general impression about the Commission's recommendations. In an area where subtleties of language may have important implications, such an abbreviated treatment, which is inevitably colored by personal perceptions, should be regarded with care. The interested reader is urged to obtain a copy of the Commission's *Report and Recommendations on Institutional Review Boards* from the Commission office (Westwood Bldg., Room 125, 5333 Westbard Avenue, Bethesda, MD 20014) or from the Office for Protection from Research Risks (see footnote two).

identifiable private information about them. This definition would include some studies that many IRBs have not reviewed in the past (e.g., studies of medical records), but it also makes clear that some studies of human beings (e.g., observational studies of public behavior) need not be reviewed.

The Commission would continue to have IRBs consider risks to subjects in the framework of the benefits of the research, although it recognized that no precise weighing is possible. As long as a reasonable relationship exists between risks and benefits, however, the Commission would not have an IRB substitute its judgment for the judgment of subjects.

The Commission endeavored to make informed consent more of a reality and directed IRBs to consider more than consent forms (Gray et al., 1977; Gray, 1975). It recognized the distinction between informed consent and consent forms, and would give IRBs discretion to waive requirements for written documentation of consent when this would unduly burden research while adding little protection to subjects. The Commission also recognized that the requirement for written consent creates a record of subjects' identities, thereby increasing the risk of disclosure to an extent that might be unacceptable in studies of illegal or stigmatized behavior. The documentation requirement can be waived on those grounds. (More generally, the Commission would increase IRBs' attention to the issue of confidentiality of data.) The Commission also made provision for IRBs to approve certain research in which information is to be withheld from subjects so as not to affect the validity of the research. Similar provisions were made for deception. There can be no deception or withholding of information regarding risk, however. There is also provision for waiving of the informed consent requirement entirely under certain circumstances, particularly in studies of records, provided that the interests of subjects are otherwise protected.

The Commission would also have IRBs maintain records of their actions on proposals and the basis of those actions. The IRB would have to inform investigators of the basis of decisions to disapprove or require modification of proposed research and give them an opportunity to respond in person or in writing.

Other recommendations of possible interest include provision for expedited review procedures for defined categories of research that present no more than minimal risk; the requirement that IRBs include persons with the competence necessary to analyze accurately and thoroughly the risks and benefits of proposed projects; the statement that approval by

a single IRB should suffice to meet regulatory requirements; and the recommendation that regulations be standardized across federal agencies.

Conclusion

The Commission's *Report and Recommendations on Institutional Review Boards* will probably have been issued by the time this article is published. (A separate report will be issued regarding the basic ethical principles for the conduct of research involving human subjects.) Under the law, the Secretary of DHEW, must publish the Commission's recommendations in the *Federal Register* within 60 days of receiving them and provide opportunity for interested persons to submit their views. Within 180 days of publication of the recommendations in the *Federal Register*, the Secretary is required to decide whether to accept the recommendations. If he does not accept the recommendations, he must publish the reasons. If he does accept them, then a revised set of regulations for protection of human subjects will presumably be proposed, and again public comment will be sought.

Thus, the next year is a crucial time for persons who are interested in this topic to comment on the Commission's recommendations or on the regulations that may be proposed to implement those recommendations. If DHEW follows the same procedures as in the past, the Office for Protection from Research Risks (see footnote two for the address) will be the source of information about the status of the recommendations and regulations, and will probably be the office designated to receive the public's comments. The ASA Executive Office plans to monitor these developments. The Commission is scheduled to go out of existence in the fall of 1978.

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In order to comment upon the issues raised in the three lead articles I find it helpful to suggest a model of sociological research that begs the softening of a series of trained incapacities, which the sociological community has worked long and hard against substantial resistance to develop. I propose that what sociologists do in experimentation, surveying, field work and interviewing is nothing more than what otherwise normal people do in living their everyday lives. Sociological research is talking to people, asking them their opinions and making notes on observations and conversations. Sometimes sociologists do these things systematically and sometimes not. Whether, on balance, they do them better than normal people is not at issue here.

I realize that mastery of this model may be difficult for sociologists who have mystified their methods in countless methods texts and have generally had to lay claim to scientific status without the benefit of the complicated hardware so useful in the bio-sciences for doing so. Nevertheless, my modest model has as its first advantage the quite immodest consequence of holding that sociologists ought not

to be restricted any more than anyone else from talking to, observing, or making notes on anyone in any way they see fit. No board or committee of well-meaning colleagues or concerned citizens should have to be consulted by any sociologist either before, while, or after he does so. Furthermore, should sociologists choose to lie, misrepresent, manipulate or renege on bargains they make in the course of talking to and watching people, they deserve no special privilege that would relieve them from suffering whatever personal sanctions such scurrilous behavior earns otherwise normal miscreants.¹ That is what the first amendment is all about, leaving sanctions for wordy abuse to civil society, and making no differentiation in doing so between the scurrilous talk, writing or observations of sociologists and other citizens.

Although the perfect freedom and personal risks the modest model promises are enough to sustain some hardy species of sociologist, that invigorating combination depends upon certain material conditions for its existence. Principally, the sociologist who wishes to be a totally independent practitioner of his craft must own his own tools and be able to make a living off of selling what he produces with them or support himself in some other unrelated way such as teaching. If he cannot do so or chooses not to because he finds the security of salaried labor more attractive, he must find some way of reconciling the freedom of independent craftsmanship with the truth that in becoming an employee his work risks not only his own but his employer's investment and reputation.

Sociologists have managed that reconciliation very well, but each of the three lead articles reveals certain strains in the bases upon which they have been able to do so. None of the articles has anything to do with the ethics of research, though each does raise, under the cover of the sacred canopy of ethical consid-

¹ Although I am vigorously opposed to getting caught, I am not categorically opposed to breaking the law in the course of research. To analyze when and for what reasons is beyond the scope of this commentary, but generally my analysis would follow Weber's lines in "Politics as a Vocation": No ethics in the world can dodge the fact that in numerous instances the attainment of "good" ends is bound to the fact that one must be willing to pay the price of using morally dubious means or at least highly dangerous ones—and to face the possibility or even the probability of evil ramifications. From no ethics in the world can it be concluded when and to what extent the ethically good purpose "justifies" the ethically dangerous means and ramifications (Weber, 1958:121). What better basis could there be for arguing for impossibility of a code of ethics for sociology?

eration, some serious problems in the practical politics of maintaining first amendment freedoms at bargain basement rates. I will comment on each of the articles in order of the increasing tightness with which they have woven this sacred canopy over what they have to say.

First, Bond's review of statutory guarantees of immunity from subpoena. Presently working under such a guarantee (granted under 21 U.S.C. 872(c)), I too appreciate the advantages of such a privilege. However, if the cases excerpted from Carroll and Knerr (none of which would rate a footnote in the history of attempts to tamper with first amendment rights), are representative of the abuse sociologists have had to withstand to guarantee their subjects' anonymity, asking for more protection than is already available would seem a bit greedy. What Bond's review omits is any consideration whatsoever of conditions under which the state might justifiably disregard a promise of confidentiality and quite properly attempt to force a sociologist to reveal in detail what has been learned. Should we gain the right of absolute immunity from subpoena we also ought to consider how awkward it will be to get a laugh out of Comte's vision of a sociological priesthood once we have achieved a confessional immunity that most states decline to grant those with genuinely vested interests.

Second, Stephenson's account of a close encounter of the third kind with the CIA. We ought not to miss the irony that the CIA's passion for confidence comes round to haunt it in the person, institution and journal of the professional association whose confidences it confidentially sponsored. Theoretically, the issue the irony raises is the relationship between confidence and confidentiality. Confidence, as any CIA agent, vacuum cleaner salesman, con man, Dale Carnegie course, or good field methods text (see index under "rapport") will tell you, is merely a matter of technique. Confidentiality, beyond what a researcher might find convenient to recall, is purely technical. If researchers keep records that expose subjects to danger or incriminate them, they (all three) can be had by people with the skills of gaining confidence or truly superior political power. If Woodward, Bernstein, Nixon, the CIA and the ASA should share nothing else, they should share that hard lesson in confidence and confidentiality.

Third, Cassell's essay on risks and benefits to the subjects of field work. This essay raises a lot of questions, at least fourteen in the body of the text and six in the *Abstract*, not counting any that are implied in declarative sentences and not signaled by a question mark. At least

three-quarters of them are unanswerable if taken seriously and doing so is quite beyond me. None of the practices Cassell cites rises to the stature of an ethical question worth policing, something she herself notes that review committees at larger universities have had the good sense not to do. There may be many reasons for this, but three seem likely to me: (1) aware, perhaps, of the failures of regulation in less subtle industries, they understood that field work was impossible to police; (2) perhaps they understood that the special intimacy, understanding, and judgment good field work requires makes it largely an ethically self-policing labor when practiced by good field workers; (3) perhaps they just could not believe in the myth of a scale for field work risks and benefits on which anybody could "weigh" or "balance" anything.²

Finally, I wholeheartedly support Cassell's call for a dialogue among ethnographers, particularly if it is stimulated by contributions from students of ethics. The whole purpose of that talk should be to keep talking, something that philosophers have done for centuries without ever reaching an unequivocal position on anything.

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² Elsewhere I have explored the defects in the application of a risk benefit analysis to field work in some detail (Klockars, 1977).

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As all three of the papers point out, the Federal government has become more active in recent years in setting regulations to protect individuals who participate in research from unnecessary or excessive risks. An underlying presumption of the regulations is that risks will be balanced against benefits in making ethical judgments. A category of "non-beneficial research" has been used to describe studies

which are not likely to benefit those persons who are the subjects in the research. Though social scientists like to think that their research is or will be beneficial, much social research actually falls into the category "non-beneficial." In the absence of clear benefits to the subjects, the amount of risk they incur must necessarily be negligible.

Benefit and risk may be incurred by the subjects in the research, the populations they represent, or the society in general. Though not addressed in Federal regulations, both risk and benefit may be incurred by the person who conducts the research as well as the profession represented by the researcher. Federal regulations came about because risks to subjects and the classes of persons they represent are often greater than many individual social scientists recognized and the various professions had failed to establish systems for monitoring the risks in research conducted by their members. The three papers discuss some of the risks to subjects, but risks occur in many more areas. Bond identifies violation of confidentiality as the greatest potential risk in social research; I think there are other risks equally as great. Stephenson describes the possibility for serious jeopardy to individuals as well as to the populations they represent when the funding source is an undercover government agency. Cassell discusses a number of potential risks to subjects either when the study is conducted or after publication of the information. She also points out that one person or group may benefit from the risks of another. It is therefore often a difficult moral as well as scientific task to weigh the risk-benefit equation and to attempt to anticipate all possible future uses of the information.

Because these ethical issues are often so difficult, the Department of Health, Education, and Welfare requires that each application for Federal research funds be reviewed by an Institutional Review Board (IRB) prior to submission, by the peer review committee judging the scientific merit of the application, and finally by the staff of the funding agency. Cassell accurately notes that there is a wide variation in the degree of scrutiny given research proposals by IRBs. As a research grants administrator for the Federal government, I annually review some 300 proposals to conduct basic social science (not intervention) research. I have found that approximately 10% of the applications have potential for considerable risk to subjects. Another 10% raise minor or moderate concern about the ethics involved. Most of these applications have been approved by IRBs. The risks include not only the one of confidentiality mentioned by Bond, but also

deception of the subjects that is unnecessary for the purpose of the research, failure to debrief subjects, interview questions that are insulting or demeaning, and research paradigms that cause the subject physical pain, embarrassment, or are potentially degrading or damaging to the self-concept. A few studies have made elaborate plans to gather data covertly, while many have simply failed to plan to obtain consent. One study had as part of its research plan revealing the information obtained from adolescents to their parents; many have planned to gather personal information not necessary to the aims of the research and for which there was no plan for analysis. When human subjects problems are identified in applications otherwise of high scientific merit, agency staff contact the investigator and attempt to work out ways of correcting these issues. Any human subject problem identified must be resolved prior to funding. Because of the relative frequency with which such problems are identified, I heartily endorse Stephenson's suggestion that more attention be paid to ethical issues in courses in research methodology.

Bond raises the very real issue that written informed consent may in fact put some subjects such as criminals, drug addicts or prostitutes at risk because the consent forms may be the only record that these persons have participated in the research; such records are not subpoena-proof. Some researchers have handled this risk by sending the consent forms to a colleague in Canada where they are maintained in locked files. The decision to fund such research is always balanced against the likelihood that the research would be of benefit to the populations represented by the subjects, and often this decision is a difficult one.

Not discussed in any of the papers is the risk to individual subjects as well as the populations they represent when spurious findings are published and utilized. Whole classes of subjects can, have, and probably will be damaged by research that is inadequate in theory or design, is insensitively or incompetently conducted, when the analysis is inadequate or inaccurate or when conclusions are drawn that cannot be supported by the data. While all social research is to some extent culturally bound, results of research based on ethnocentric assumptions, on tests that were not culturally sensitive, on studies which did not consider race or sex or socioeconomic status as variables, have been used to make social and academic policies to the detriment of more than one generation of Americans. To be sure, science can only be judged by the state of the art of the time, but at times that state has been a sorry one. It could

be argued that social science theory and methodology have advanced as much because of the criticisms of such problematic research as in spite of it. When research proposals are reviewed for Federal funding their scientific merits are considered separately from the ethical issues. However, when consideration is given to possible uses of the findings, scientific and ethical issues become inextricably entwined. Courses in research methods should include discussion of such considerations.

The Federal government does not review possible risks and benefits to researchers, but the issue is of professional concern and does influence the risk-benefit equation. Risks to the social researcher are not as common as risks to subjects, but Stephenson has provided an uncomfortable personal example of such a risk resulting from deception of the researcher by the funding source. His paper should sensitize researchers to the need for openness to subjects about the source of funding. Surely subjects have as much right as researchers to know who is paying for the information they are providing.

In general, the risk-benefit ratio is rather heavy on the side of benefit to the researcher. Social scientists may gain experience, obtain a degree, profit financially or gain in professional prestige from conducting research. Scientists tend to benefit more when their research is competent, but they may even benefit when it is not. This potential for personal and professional gain from the conduct of research makes it especially difficult for individual researchers to be objective about the potential hazards to subjects and populations.

In making ethical judgments about research, the risk-benefit model is a complex one. What is risky for the individual may be of benefit to the population in general. Social research that has no risk to individual subjects may jeopardize whole groups of persons when the research becomes public. Research that advances the state of the art in a particular field may have negative consequences for the populations studied, or the individuals who participated in the study. In assessing ethical values in research, often what is good for the goose is not necessarily good for the gander.

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The full scope of the CIA's covert research activities and deceptive and secret funding of research projects is not now and may never be fully known. It is thus impossible to analyze completely the implications of the CIA's activities for our profession. Nevertheless, Professor Stephenson's description and discussion of his unwitting involvement in these affairs is a valuable beginning for a fuller study. I too was a dupe of the CIA—I received financial support for a project from an agency since identified as a "front." My experiences are different enough from Stephenson's to alter somewhat certain of the ethical consequences which he explores.

I first learned of the Human Ecology Fund in 1962 from two then colleagues at Indiana University, Bloomington, both of whom were conducting research supported by that source. I wrote informally seeking funds for some research plans of my own. The executive of the Fund responded that he had "circulated (the) proposal to some . . . consultants . . . (who) evinced a positive interest." He enclosed a description of the Fund and instructions on the format preferred in the preparation of formal proposals.

The formal proposal was submitted in the customary manner by the Indiana University Foundation, legally the recipient of the grant. I was specified as the principal (and only) investigator. Within a few weeks the project was funded, but for Phase One only (the field work phase). The Fund's "Board of Directors" had "decided to withhold consideration for support of Phase Two (analysis) until a later date" when they could review an interim report for Phase One. While in the field (in New York) I encountered some unanticipated expenses and I telephoned the Fund (also in New York) to request additional money, which was granted. I submitted the interim report to the "Board of Directors," who responded favorably with funds for support of Phase Two. As a final report I submitted a copy of a paper on the research, which I also delivered at the Annual Meetings of the ASA in 1965. I never heard from the Fund again. After the disclosures concerning the CIA in 1977 I tried to contact the Fund, but it was no longer listed in the New York telephone directory.

The foregoing paragraphs outline *all* of my contacts with the Human Ecology Fund. Except for the one telephone exchange, all of the communications were written. At no time did anyone from the Fund make any suggestion or comment of any nature on any aspect of my work. I drew up the schedule of questions, and the Fund did not see the questions until it re-

ceived the interim report after the field work was concluded. I hired and trained all the interviewers. The Fund showed no interest in the raw data or in the interview records, so there never was a possibility of social risk to subjects, as in Stephenson's case. As this record indicates, all of these transactions were conducted in accordance with ideals of ethical responsibility and academic freedom. The Human Ecology Fund was in no way distinguishable from any respectable funding agency. [An announcement from the fund is reproduced on p. 182. Ed.]

If the conduct of the agency gave no hint of links to more nefarious activities, the funding of my project was equally disarming. I proposed a restudy of a well-known suburb of New York City, which had been the subject of my dissertation a decade earlier. The restudy was to investigate population turnover and its micro-ecological consequences, particularly for neighboring and participation in voluntary associations.

I have no idea why this project was supported by a front for the CIA. Perhaps the "Board" or its officers were so thoroughly immersed in "mind control" activities that the attendant loss of critical sensitivity led them to read such implications in any research, including what I was doing. Maybe the "Board" believed that the support of some innocuous studies would "cover" its other activities. It is even possible that the agency needed to spend uncommitted funds before the end of the fiscal year and the funding of my project was simply an expedient outlay.

Professor Stephenson makes the point, among others more relevant to his situation than to mine, that we need to be more careful in the future about taking things at face value. The episode which I have described shows how thoroughly deceptive a funding source can be. Stephenson suggests that we need to develop some "reasonable controls" on funding agencies, with institutionalized assurances that the agencies will abide by them.

I would like to think, as I once did, that it is our integrity as researchers that will best protect us and society from perversions of the research process. But I see in my own experience that although the research I conducted was not contaminated by the tainted money that supported it, the very fact that it was carried out demonstrates that individual integrity is not enough.

Like Stephenson, I feel "had" and I am more than simply annoyed. I was not only "had," I was also "used" because in some way the funding of my project was indirectly supportive

of the full thrust of CIA research. We do need the protections that Stephenson mentions.

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Social scientists would be well advised, I suggest, to look on ethicists and Institutional Review Boards (IRBs) with the healthy skepticism most people reserve for revenueurs and the fuzz. They are all there to serve you and the greater glory of the common good, but, too often, with such friends you don't need enemies. I speak as a chairman of an IRB who recently sat through a two-day meeting of ethicists gathered partly, it appeared, to savage Stanley Milgram (also present), and more important, to celebrate the emergence of a new profession, fantasies of which set visions of jobs, journals, colloquia, and colleagueship dancing through the assembled heads.

Cassell is looking in the right direction when she distinguishes between medical-bio-physical research and social research, but she lacks the courage or wisdom to push through to the proper conclusion. With rare exceptions the studies of human behavior conducted by anthropologists, sociologists, psychologists, economists, and political scientists do not involve the possibility of significant harm to their subjects. Their subjects are not "at risk." I say "with rare exceptions" because it is impossible to demonstrate that none exist and yet, in truth, the exceptions are hard to find outside the margin where biology and psychology overlap. Social scientists have been remiss in not insisting on this patent reality and challenging those who so eagerly press for needless, senseless regulation. The regulatory game is not worth the candle and a sixth-grader with common sense would see that the cost-benefit balance of all that reviewing and approving and informed-consent-documenting is wildly against the rules already on the books, let alone those that are proposed.

In 99% of the cases, the worst that could happen in the small area of social research which contains the possibility of harm at all is slight embarrassment, bruised egos, umbrage, and ruffled vanity. This is a level of trauma that adults, college sophomores, infants, blacks, whites, the educated and uneducated, the rich

and the poor are able to sustain at no great cost. Heaven forfend that I should be understood to advocate that social scientists go about embarrassing folk, bruising egos, eliciting umbrage or ruffling vanity. I do not. A heavy responsibility lies on the professions to do all they can (in recruiting, training, certifying, and rewarding scholars) to ensure that members will try to avoid such consequences of their research. Most of them do.

The fact that this plea for unregulated professional responsibility is familiar within the biomedical community and that it is an inadequate argument there, does not make it similarly inadequate for social science given the differences in the fields and the different possibilities of harm to their subjects.

As Bond suggests, the need for privacy and confidentiality in certain kinds of social research is a more serious problem. Yet I suspect it is the investigator's problem (and the field's) more than it is the subject's. When an honest investigator cannot give subpoena-proof guarantees of confidentiality, it is hard in some areas of research to recruit willing subjects. The stately progress of science is impeded. Is that such a bad thing? I am not persuaded that society is well served, in the long run, by opening to social science a privileged window on our souls and behavior. Perhaps investigators ought to learn to cope with the inhibition and self-protectiveness people display about revealing certain kinds of information. The temptation to fashion a situation in which the social scientist and putative subjects can step outside society (time out, fingers crossed) is understandable, but are the benefits to be gained by giving social science so special a role sufficient to justify the predictable problems? The news-hound's sources are not and should not be legally privileged, in my view, though society is well advised to minimize occasions of forceful intervention; social scientists should accept a similar ambiguity in their relations with informants and society, summoning some of the courage newsmen have displayed when the dilemma appears.

Finally, Mr. Stephenson, gulled some twenty years ago, should resist the impulse to seek feeble remedy by proposing further bureaucratic epicycles to protect us from the spooks. The intelligence agencies exist and most of us reluctantly acknowledge their necessity much as we also regret it. When they must, they will continue to lie and cheat and steal in our best interest. Congress and the administration should do what they can to ensure that it is, indeed, in our best interest. Individual scholars approached for help must consider what is right in terms of the circum-

stances they confront. I would hope that all concerned have now concluded that spook-sponsored "fronts" that use social scientists unwittingly do more harm than good in the long run. If not, neither Stephenson nor the ASA can do beans about it and you and I may find, years hence, that we too have suffered Stephenson's disagreeable experience. The mechanism he so solemnly suggests to protect us won't.

Obviously, I have been cavalier about important problems which exist. I am reacting to what seems to me an excessive sensibility displayed by many within the behavioral sciences. Among our responsibilities is that of helping to formulate sound public policy in an area we know well. To accept, to the extent we have, the inappropriate bio-medical model as relevant to social science subject protection is to mislead public and politicians—and ourselves.

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I was Richard Stephenson's partner in "MKULTRA, Subproject 69." More than he, I had sensed a research opportunity. I had run around seeking access to the Hungarian refugees streaming into Camp Kilmer and was digging into the literature on the sociology of revolution, a subject that had beguiled me for some time.

Although an abstract Marxist and something of a graduate-student activist (I had been reading Marx for a year at the London School of Economics before joining Stephenson and Harry Bredemeier at Douglass College; I was one of Robert Lynd's last Ph.D. candidates at Columbia University), I was as elated as Stephenson when Jack Riley, then chairman of the Sociology Department at Rutgers University, directed a \$5,000 grant to Stephenson and me from an unfamiliar and esoteric-sounding foundation called the Society for the Investigation of Human Ecology.

As Jack and Matilda Riley recall it, one of them had taken a call, quite out of the blue, from James Monroe, an ex-air-force-officer who had been a behavioral science manager for the air force during the last stages of World War Two and the beginnings of the Cold War. Monroe, the director of the Society, and as it turned out, a CIA operative, asked the Rileys to attend a meeting to discuss their participa-

tion in a multidisciplinary study of aspects of the Hungarian Revolution. The study was to be funded by the Society. It was to be directed by Drs. Harold Wolfe and Lawrence Hinkle of the Cornell Medical College, and based at that institution.

The Rileys were enthusiastic about the idea of such a study. They initiated some preliminary inquiry, and reluctantly concluded that neither they nor their research group could, at such quick notice, make the major commitment of time and energy that the Hungarian project required. Knowing that several of us at Douglass had been trying to start our own project on the sociology of the Hungarian Revolution, the Rileys recommended that the Society make the grant to Rutgers University, and that Stephenson and I do the study.

I wanted to do the work I wanted to do and simultaneously earn my Ph.D. by writing a compatible thesis. I was glad for the grant, but much more excited by the research access and opportunity for collaboration it provided. I would have gone to work with the Cornell group had there not been a penny available from the Society. It simply never occurred to me to think about the source of funding, let alone question it. It was enough for me that I was going to have a chance to do what I wanted to do.

I saw myself as developing a theory of revolution or, at the very least, refining Crane Brinton's *Anatomy of a Revolution*. I would write a dissertation worthy of the intellectual I wanted desperately to be.

So I was incensed when Lynd and Sigmund Diamond officially rejected my dissertation proposal. I saw their decision as a failure of nerve, as *their* old left paranoia. Lynd warned me privately that the study I wanted to do would be of practical significance to the State Department and other government agencies and that no respectable analytic study could or should be based on self-serving refugee data. I countered that the 1956 uprisings in Eastern Europe were important sociological events and that their explanation could and should be fitted into a broader analysis of revolution.

I was unwilling and unable to listen to Lynd for several reasons. (I had no connection with Diamond.) First and most important, his failure to create a real, critical Marxist alternative to the functionalism taught by Robert Merton, and his inability to do more than spit at the main-chance empiricism of the Bureau allowed me to discount his advice. Second, Lynd's hesitant and wavering public commitment to socialism offended me. Socialism, it seemed to me, was clearly preferable to capitalism.

I have told how I came to be working for the

CIA in 1956-57 in order to illuminate the different types of circumstances which lead to abandonment of moral and political responsibility. Stephenson suggests that he was naive; I was obviously intellectually and politically arrogant; the Rileys had come to see the social sciences as policy sciences which should serve the state in a capitalist democracy and in turn be generously supported by public funds through state agencies.

I dwell on personal and social responsibility because we sociologists know well enough that knowledge is not neutral: that different groups, organizations, and movements have different amounts of power to obtain and use information; that the choice of research problem, form and type of dissemination, and audience is dictated by a combination of values and position as well as idiosyncrasy. The sociologist who has not measured his own values, who has not faced the implications of his own research choices and affiliations, is as dangerous to the survival of the intellectual life as is secret research.

The CIA, in a less than perfect world, has every right, a responsibility even, to try to fund external studies of unanticipated or surprising events occurring outside the United States. Certainly, the CIA has as much right to gather accurate information in its mandated area as do other government agencies in theirs. However, all such research should be openly sponsored because secrecy corrupts democratic and intellectual institutions and because open research produces better information and better analyses.

The CIA could engage in openly sponsored research by contracting with individuals who might or might not be institutionally affiliated, or by making grants directly to or through academic institutions. If university officials and faculty are squeamish about taking monies openly from the CIA when there are no strings attached, let them distinguish such funds from HEW, or NIMH, or LEAA, or Ford Foundation, or Exxon grants. If the issue is that CIA monies aimed at a particular area or line of inquiry would direct scholarly energies into that problem thereby structuring free inquiry, isn't that what happens all the time anyway? If the issue is that scholars choosing to work on CIA problems and grants must be protected from the consequences of that decision, then there is something very wrong with what the CIA is doing.

Stephenson's procedural proposals appear to be directed at mitigating the effects of manipulation of the powerless and the less powerful by the very powerful. Standards to protect the rights of human subjects, enforced by

committees of professionals and bureaucrats, may protect the rights of the very powerful even more than the powerless. I become indignant contemplating the courses in professional ethics that the already implicated professors of sociology would in good faith offer their graduate students. We sociologists need to get at the root system of secret research and our own complicity. When we are able to do so, the "solutions" and strategies proposed may be more than palliatives.

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Editor's note: Since Dr. Schulman specifically mentions John and Matilda Riley in his comment, I asked them if they wished to respond to the comment. They provided me with the following:

To the Editor:

Indeed, we were all taken in!

It was a most unusual opportunity to bring sociological research to bear on a medical study of a population under extreme stress. None of us, however, had any reason to doubt the integrity of those who shared our scientific and human interests in the enterprise.

John and Matilda Riley

The American Sociologist is to be commended for its publication of this series of essays focusing upon moral issues related to the research process. Each of these papers provides insight into ethical issues that need to be reflectively considered.

Before examining specific issues raised by these papers, we shall begin with some general observations on the state of sociology in the areas of ethics and human rights. We find it informative that, although moral issues are debated in *The American Sociologist*, they are typically ignored in substantive articles appearing in journals such as the *American Sociological Review*. If we take this crude empirical indicator as a guide, it appears that a number of sociologists believe that issues relating to ethics and human rights merit attention from the profession, but that they are extraneous concerns in the serious business of reporting scholarly research. The essays appearing here, however, suggest strongly that questions of ethics and human rights are present in almost every facet of the research process, including the theoretical framework and the evaluative presuppositions of the methodology employed.

The essay by Cassell points to some informative areas for debate. She takes the risk-benefit notion as the starting point for investigating certain issues in social research. While making an important distinction between experimental and field research with respect to this issue, Cassell does not examine the theoretical nature of the risk-benefit criterion. This failure is widespread among social scientists.

The risk-benefit criterion represents a form of utilitarian reasoning, a position that argues for the calculation of pleasure over pain as a moral guideline. In its broader expression, activities are judged in terms of the "greatest good for the greatest number" standard. But this standard typically locks one into the categories of the existing nation-state system, and such an orientation gives less than adequate attention to minority perspectives.

Cassell's discussion suggests that utilitarian theory is an inappropriate guide for action, at least in particular kinds of research. The principle of reciprocity would seem to call for other theoretical standards, but Cassell has not articulated these, for she, like most social scientists, operates on a taken-for-granted level when analyzing the moral issues that arise in social research. Works such as Rawls' *A Theory of Justice* (1971) and Dworkin's *Taking Rights Seriously* (1977), among others, should make sociologists increasingly aware of the theoretical foundations of their own actions with respect to ethics and human rights.

Stephenson's recounting of his own experience with the CIA is instructive to sociologists in other respects. It seems likely—given what we have learned from Watergate and its fallout—that the major ethical problems in research derive not from individual researchers but from large-scale bureaucracies, which can manipulate social scientists as well as the information they collect.

More specifically, sociologists frequently acknowledge the need to respect subjects' rights in such areas as privacy and confidentiality. But how can this be done when large-scale organizations can and do from time to time utilize their hidden or secret sides to destroy the very rights that social researchers claim to protect? Bond alludes to this problem area but fails to give it the special attention it deserves, for she makes no distinction between the powerful and the powerless with respect to privacy and confidentiality. In our judgment, sociologists must do more than discuss general moral orientations; they must pay special heed to how these are implemented (or undermined) in everyday life by large-scale bureaucracies that have as an integral feature a hidden side. Soci-

ologists concerned with ethics and human rights must develop far greater understanding of the role of the powerful bureaucracy in defining moral issues in modern society.

Overall, these essays reinforce our own view that the researcher and the institutional setting are significant factors in the very design of research—in, for example, the statement of the problem, the kinds of research procedures employed, and the verification principles invoked. Researchers, in fact, have their own moral orientations, largely unarticulated, which tend to structure the results of any research endeavor. Thus the resolution of technical issues alone will not suffice to help us construct a methodology that will take account of the moral concerns raised by these three essays.

These essays, along with others more informed by theories of ethics and human rights, should help sociologists who are confronted by the Federal Government, particularly through DHEW guidelines, to take moral issues into account when designing a project. (At the same time we must be careful to distinguish legal regulations from issues relating to ethics and human rights.) Only when the impact of the researcher upon the overall research design is fully appreciated will we be able to transcend the dichotomy between "professional sociology" and "scientific sociology." If ethics and human rights are taken seriously, the nature of social inquiry will be restructured and so will many of the articles published in leading sociological journals.

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REJOINDER

The responses to the papers illustrate very well Berreman's observation that ethical problems in research are complex, highly contextual, and controversial. For this reason, as well as space limitations, my comments are brief and selective.

I can agree with Pattullo's broad generalizations: that social science research (considering its volume and variety) rarely involves the

possibility of harm to subjects; that most scholars try to avoid such harm; and that their protestations often involve excessive sensibilities. I do not agree with his conclusions. As I understand him, Pattullo believes that law-abiding people should be unconcerned with "due process" because they have nothing to hide or fear. Whatever the balance of harm and good in research, it always is prudent to avoid unnecessary harm and to recognize that harm takes many forms and may involve more than the subjects of research. Pattullo and others note the danger of placing so many bureaucratic conditioners, strictures, and constraints on research that few will want to fund research or engage in it. All the more reason, then, to explore ethical questions, give them reasoned consideration, and communicate about them.

A significant step in this direction is Berreman's mention of the need for a data base of examples and greater awareness of and information on the particulars of ethical problems. Professional associations can be instrumental in this respect, helping to insure that "informed initiation and design" of research will meet the responsibilities of people engaged in it. However, we must remember that research often involves agents other than researchers and subjects. These agents also have responsibilities and obligations, which, if not observed, may infringe on those of researchers. Researchers must have some assurance that others will also observe their obligations.

Response to this problem may take the form indicated by Douglas's "ordinary human being" (most of us are), Schulman's getting at the "root system," or the "you can't fight city hall" attitude implicit in other comments. However, a disciplined chorus ordinarily speaks louder and clearer than a cry in the wilderness; "getting at the roots" needs at least specification if not a millenium; and defining a situation as real is real in its consequences. Surely there are other solutions to the problems if they are sought, and we are not so oppressed that we cannot take them. However, we should not be arrogant in our search for solutions to our problems.

Schulman observes that even the CIA has the right to openly funded research. Lazar remarks on the dangers of incompetence and "spurious findings." Klockars asserts that social scientists are not immune from intervention and must be prepared to pay for risks deliberately taken. Vaughan and Sjöberg urge researchers to a self-examination of their moral orientations. All are points well taken.

The first paragraph of Gray's paper states succinctly the need for discussion of the proper

role of government in the regulation of research. His statement applies equally to other roles in the research enterprises, where governmental regulation may not be relevant. Social scientists cannot be totally free agents, nor can they pursue research without some consent and support. Carroll mentions the apparent indifference of social scientists to these hard facts and exhorts professional and collegial associations to heed them. Friedson indicates the sort of action individuals can take. Coser outlines the consequences of indifference and unconcern, which may well be the ultimate results for research and scholarship if these ethical problems are not faced squarely, and soon.

Richard M. Stephenson

REJOINDER

The United States has been called "a nation of moralizers." Douglas, Pattullo and Klockars would agree with this critique when considering the recent federal attempt to regulate the ethics of social science research. The arguments of these commenters are presented with such polemic, skepticism and wit that we might judge those who worry about harm to subjects of social research to be afflicted with the sin theologians call *scrupulosity*: excessive agonizing over small and unimportant transgressions. (A friend, who hears confessions and counsels young priests on how to be confessors, tells me that scrupulosity presents great problems to those who must counsel and comfort people who are constantly tormenting themselves over unimportant, imaginary or non-existent sins. "We used to think they should be sent to a psychiatrist," he said, "but we discovered that *they* can't do anything for these people, and they're bounced right back to us.") Douglas, Pattullo and Klockars apparently believe that the worst that can happen to the subjects of social research is "slight embarrassment, bruised egos, umbrage and ruffled vanity" (168). Their appraisal transforms the regulatory effort, and ethical concern itself, into objects of ridicule, so that those who care about the ethical quality of social science research are in danger of being classified as cranks, bores, or people suffering from terminal cases of scrupulosity.

Bererman, Stephenson, Schulman and Liell might seem to answer these three critics, since they do believe that social researchers have indeed been responsible for much harm to their subjects. Unfortunately, however, a hasty perusal of their arguments might convince an

innocent reader that this harm was performed not by the researcher, but by the CIA, the military, or other instrumentalities of imperial America. If both interpretations were correct, we would be confronted with the ultimate Catch-22. We would have to conclude that, having inflicted major damage on various innocent peoples, the federal government is now responding to the national penchant for moralizing by empowering commissions to impose ethical regulations upon social researchers, a group whose error (or crime) has been that some have been seduced by (other) governmental agencies (notably the CIA and the Defense Department) into providing data, which have facilitated harmful activities by those agencies. Alas, this is a seductive, but simplistic formula which gets social scientists off the hook by transforming them from active if occasional agents of harm to unwitting dupes.

We researchers know we are good people. Consequently, we take it for granted that we would not and could not harm those we study. Or, as Klockars puts it, "the special intimacy, understanding and judgment good fieldwork requires makes it largely an ethically self-policing labor when practiced by good fieldworkers" (165). By "good," I should guess that Klockars means "competent," but apparently he believes that in doing fieldwork, technically *competent* and morally *good* are identical, or at least interchangeable. Unfortunately, many social scientists still use the term "good" in this elastic sense.

Unhappily, the activities of federal regulators, and their local offspring, Institutional Review Boards, have frequently been sufficiently misguided or irrelevant so as to confirm some researchers in their feelings of abused righteousness. We are faced with what might be called ethical overkill (or institutional and governmental scrupulosity). There are many examples; I note one in footnote four of my article (p. 141). In another, Queens College, in New York City, has just passed a set of review procedures that would require that, for each student research project, the teacher would have to steer a protocol through two review committees, providing each with a statement and an analysis of "risk/benefit." Since it is clearly impossible for one person, in the space of a single semester, to help students to learn about, formulate and conduct research, as well as helping to develop project descriptions and calculating risk/benefit ratios, to be moved through the soggy review of two committees, the teacher has the choice of ignoring the regulations or teaching by describing rather than by doing. These "ethical regulations," then, force

teachers to be in violation of university rules, or to deny students the opportunity of firsthand experience with fieldwork and with its ethical problems.

The standard apologetic of wrongdoers is that they are only doing in a small and ineffectual way the same thing that others have done more massively and terribly. When political judgments of the United States as a capitalistic nation characterized by an imperialistic world policy are confronted with self-excusing moral evaluations of researchers, then researchers are inevitably going to appear as simple dupes or white knights. We would do better to attribute moral responsibility to researchers: to see them as responsible adults whose actions exist in an ethical space where decisions must continually and fatefully be made. Because federal regulations can be used in ignorant or unethical ways, there is a temptation for researchers to retreat into a kind of moral nihilism, and conclude that regulations are unnecessary because good social scientists are ethically self-policing. Our collective sin then becomes *hubris*. Of the two, scrupulosity is probably preferable.

Clumsy as are the federal regulations to protect human subjects when compared with the realities of social research, they are based upon the idea that there cannot and must not be a differential in status and treatment between those who experiment and those who are experimented upon, between those who study and those who are studied. We can no longer afford academic or intellectual colonialism, which takes for granted that the researcher knows what is best for those subjected to research. No matter what their methods, too many social researchers have in fact accepted this differentiation between the doer and the done to, and benefited from it, if only passively. Although the dynamics of certain kinds of ethnography seem to guard against such differentiation and exploitation, it can still occur; and it is all too easy for researchers to hide behind the simplistic notion that we are good people, we are different, and those who are trying to regulate our research are officious meddlers.

As Gray notes, whether we like it or not governmental regulation of social research is a fact of life, unlikely to change in the near future. Thus, it becomes an academic exercise to wonder whether or not research should be what Carroll calls "a regulated industry." Instead, we must begin to think about what kind of regulations would be most appropriate. To do this, we need more than wit or polemics. We need, as Berreman notes, a "data base of examples of actual harmful consequences of

anthropological [and sociological] research" (153). Although there has been discussion of whether regulations are necessary, and whether people are harmed during the course of social research, there has been, with few exceptions (Appell, 1976; Rynkiewicz and Spradley, 1976), a dearth of concrete analyses and case studies differentiated according to research methods. It is time for social scientists to start to do what they like to think they do best—to gather empirical evidence to bear on the problem in hand. The collection and analysis of case studies should help achieve Berreman's goal of "enhanced awareness of and information on ethical problems in research: consciousness raising and acceptance of responsibility and accountability" (154). It should also help us evaluate various ethical frameworks, such as the risk-benefit criterion, and discover what kind of practices, regulations and teaching will help improve the ethical quality of social research.

We have reached the point where ethical analysis and regulation of social research are hampered by their lack of specificity and concreteness. Each strategy and method of social research has its distinctive relationships between those who study and those who are studied, with its associated moral dilemmas. As yet, there is little empirically grounded analysis of these relationships. I, myself, am committed to such an analysis of fieldwork. I invite other ethnographers to work with me, and urge those who use other methods to carry out similar reviews of the ethical problems associated with their fields.

Joan Cassell

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REJOINDER

The Need for Confidentiality

Why is confidentiality considered an important principle by social scientists? I will suggest two reasons. First, a concern for the "autonomy and integrity of persons," which Bradford Gray points to in his discussion of the conclusions of the Commission for the Protection of Human Subjects, dictates that researchers should not divulge information about

respondents. Persons chosen to participate in a research project should not be exposed to the possibility of having information about them used for other than research purposes; similar persons not chosen to participate in research projects are not exposed to this risk. The professional organizations representing the social sciences—psychology, sociology, anthropology, political science, and others—include statements in their codes of ethics about the importance of maintaining confidentiality. These statements are based on the disciplines' avowed respect for persons and for individuals' rights to privacy. To my mind, and I believe to most of us, this is generally reason enough for making and standing behind assurances of confidentiality.

Second is a practical concern. It is usually assumed that people will not participate in social research if they suspect that their names will be published in research reports or that information about them as individuals will be passed on to other persons for decision-making or other purposes. A recent study by Eleanor Singer (1978) at the National Opinion Research Council (NORC) gives a notion of the effect of promises of confidentiality in survey research. One-third of a sample was told nothing about the confidentiality of replies, one-third was told that responses would remain completely confidential, and one-third was given the qualified assurance: "Of course, we will do our best to protect the confidentiality of your answers except as required by law" (Singer, 1978:146). Analysis shows that promises of confidentiality have a consistent effect on nonresponse to individual questions, particularly questions on sensitive topics, but no significant effect on participation in the survey as a whole or on response quality once the respondent decides to answer a particular question.¹ In several cases, the qualified assurance elicited

higher rates of nonresponse to sensitive questions than no mention of confidentiality at all. According to Singer's findings, promises of confidentiality increase the likelihood of getting responses to individual questions. Of course, such promises serve to protect persons who are open and frank.

Thus confidentiality is important to social researchers on ethical and practical grounds. Ethical concerns focus on the rights of research participants. Practicality focuses on the quality of social research that relies on representativeness and high response rates. In pragmatic terms, confidentiality is as much the researcher's and the profession's concern as it is the respondent's, as Pattullo points out.

Statutory Protection

Does concern for confidentiality warrant protection by statute? Comments on the confidentiality issue by Freidson, Klockars, and Pattullo suggest that responses to this question should consider, respectively: alternative means of protection; whether confidentiality should be absolute or qualified; and consequences of granting social science research a special status under the law.

Alternatives to Statutory Protection. Freidson suggests a legal alternative to statutory protection: the immediate destruction of all identifiers once data are collected and verified. This would immediately preclude the use of data on individuals for other than the original research purpose, whether by legal authorities or other researchers. However, a great deal of longitudinal research is currently being conducted and even more is being called for. Panel studies, follow-backs, and follow-ups require the use of individually identifiable data. Sociological theories and hypothetical models increasingly stress change. Tests of hypotheses about human development, status transitions, and socialization, for instance, call for studies involving individuals over time—studies that must necessarily retain identifiers. The discontinuance of studies that link data from different sources would be a giant step backward in social science research methodology.

Another alternative is to refuse to cooperate with legal authorities who may subpoena research data. This method of assuring confidentiality is against the law. On the other hand, it does leave the decision up to the researcher, and solely in the realm of traditional professional and scholarly ethics. Some social researchers have used this alternative and others would probably elect it in lieu of further interference with research by the state. However,

¹ In the NORC survey, nonresponse was greatest overall for the question on earned income last year. Nonresponse to this question was greater than that for "Have you ever smoked marijuana three times per week or more?" "Have you had intercourse in the past 24 hours?" "Have you masturbated in the past 24 hours?" This finding has interesting implications for what people find embarrassing or potentially damaging. As many interviewers have learned, people are often incredibly open in the interview situation. This suggests to me that Lazar's concern about asking embarrassing questions may be excessive. Notions of what is embarrassing change over time and from group to group and individual to individual. Also, survey respondents always have the option to refuse to answer specific questions or to participate in a survey at all.

many may not relish the idea of going to jail to protect research subjects.²

Absolute Vs. Qualified Confidentiality. Though some researchers probably believe that research data is justifiably subject to subpoena, the Privacy Protection Study Commission rejected the subpoena of data in most cases, concluding that "the use of individually identifiable research and statistical records for administrative, regulatory, or law enforcement purposes encourages abuse of the expectation that information will remain confidential" (1977:568). However, its recommended statute does not grant an absolute limitation on disclosure or use by authorities. The Commission's recommendations include four instances that would allow disclosure of data about an identifiable individual: (1) when the sponsoring agency believes that the disclosure "will forestall continuing or imminent physical danger to an individual"; (2) when the researcher or sponsoring institution is suspected of violating the law; (3) for federal audits required by law; or (4) for archival storage.

The phrase "to forestall continuing or imminent physical injury to an individual" is open to broad interpretation, and the government agency is the interpreter. The Privacy Commission cites "the researcher's moral and legal obligation to report acts of interpersonal violence [he or she] either witnesses or can reasonably anticipate" (Privacy Protection Study Commission, 1977:579). One assumes that the phrase would be interpreted narrowly. Still, this provision raises many questions. Would the researcher be responsible to the governmental agency for reporting continuing or imminent physical violence? Is the researcher outside the law if he or she does not make such a report? What redress does the research participant have if the agency wrongly expects physical violence to occur? How is one to define imminent physical injury? My own opinion is that an individual may have a moral obliga-

tion to report an act of physical violence that he or she witnesses, but that anticipating violence is too subject to individual interpretation. One person may view a situation as dangerous while another may not. In this instance, I agree with Douglas's claim that moral responsibility cannot be legislated.

The Privacy Commission's second exception to nondisclosure—research data may be released if it is needed to prove illegal actions on the part of a researcher or a sponsoring institution—seems reasonable to me in light of the fact that the information could not be used to make a decision about the research participant. The Commission report cites the case of CIA-sponsored LSD research and researcher fraud as an instance when release of data, if it provides the only evidence that wrongdoing occurred, is appropriate.

The release of information for government audits, the third exception, is justifiable as long as the information is not used to make decisions about individuals. If the information is expected to affect social research participants, auditors should collect their own information openly and independently.

The fourth exception—releasing data to archival storage—also seems reasonable if a sufficiently long period of time is allowed to elapse before data are sent to storage for further use by researchers, including biographers. In general, I do not believe that a protective statute for research data need be absolute.

Consequences of Statutory Protection. Some of the problems Pattullo considers are illuminated in the context of the Privacy Commission's recommended protective statute. That recommendation would add only one paragraph to the Privacy Act, but would revise many other sections of the act in ways that I believe may hinder the data collection process. For example, the revised Act specifies the types of information that "shall be made available" to research participants by researchers, including: the authority for soliciting the information; whether participation is mandatory or voluntary; the purposes of the data collection; routine uses to be made of the data including the possibility of recontact; types of information, if any, used to verify the data; the title, business address, and telephone number of an official who can answer participants' questions; the possibility that the information may be disclosed for additional research and statistical purposes; any requirements for other disclosures; and that the individual will be promptly notified if required disclosure is made for other than research purposes. I suspect that provision of all this information before data

² The Privacy Commission notes that protection of research data may come from any of three types of sources: administrative, constitutional, or legislative. Freidson points out that journalists have not been supported by the courts in their efforts to protect the identities of informants. But journalists are not working under statutory protection; their arguments are based solely on their First Amendment rights to freedom of the press. Statutory protection changes the issue from one which posits the independence of press and government, to one which posits the right to privacy for those who cooperate in government-sponsored research and eventual planning and problem-solving activities. The latter issue is more akin to "turning state's evidence," or "use immunity," than to freedom of the press.

collection begins would lower response rates and bias research findings even more than no assurance of confidentiality.

On the other hand, one further revision of the Privacy Act would increase researchers' access to individually identifiable federal records for research purposes, provided that the federal agency finds the proposed use consistent with the original data collection, that the research purpose cannot be reasonably accomplished without the information, and that the research objective warrants the risk of additional exposure. The researcher would be required to take steps to assure the integrity and confidentiality of the records and remove or destroy identifiers at the earliest possible time. Further disclosure would not be allowed without the agency's authorization. The researcher would have to sign a written statement of his or her understanding of and willingness to abide by these conditions.

The Privacy Commission has made its recommendations to the Administration and to the Congress. If the Commission's recommendation for the protection of research data is rejected (or if no action is taken on the recommendation), a great deal of federally sponsored research will remain subject to subpoena. If the recommended protection is accepted, social researchers will have a statute that many will be comfortable with, though it is not without faults (to my mind). However, should all the recommended revisions to the Privacy Act be accepted, including those concerning types of information to be made available to subjects, social researchers will be faced with increased regulation of the research process. In principle, I support the idea of protection of research data from subpoena. In practice, the package that the protection comes in may stipulate overregulation of research. Especially in light of the favorable and reasonable recommendations of the Commission for the Protection of Human Subjects that Gray reports, which have recognized some of the special characteristics of social science research, I think that further regulation is unnecessary. Federal grantees and contractors should not have to face regulations of Institutional Review Boards and another set of requirements (beyond statutory protection of data) by the Privacy Act. If Gray is correct that DHEW human subjects regulations are likely to be adopted by other federal agencies, the review that research protocols will receive under human subjects guidelines should be sufficient to judge ethical considerations in the research process.

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A COMMENT ON LEWIS'S "WRITERS OF THE ACADEMY, UNITE!"*

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Concerning Lionel S. Lewis's "Writers of the Academy, Unite!" (*TAS* 12:176-181), the editor comments: "his view is an important one and . . . he should be provided with a forum from which to present it." The *issues* Lewis addresses are important, and his "view" is definitely controversial. The importance of the view, however, depends largely upon the extent to which its presentation follows canons of logic and rules of evidence. Unfortunately, Lewis's essay is somewhat disappointing in this respect. Convincing empirical support for his conclusions is missing. The scattered evidence he presents does not adequately support several of his contentions. Thus, while I agree with the spirit of Lewis's essay—the fundamental importance of research productivity—I nevertheless feel that his argument suffers from empirical and theoretical weaknesses.

One example of evidential shortcoming is the statement that "Publication, in fact, continues to engender the same sort of skepticism that it has since it ostensibly became a fact of academic life somewhere around the turn of the century" (p. 176). With a similar lack of documentation, Lewis contends that the manuscripts received by journal editors are "mostly intellectual remnants seeking their level, not publications" (176). I assume that this uncharitable generalization applies only to the work of others. Statements such as these are unnecessary and insufficient as evidence.

Further, I doubt that the thesis that there is a

* I thank the following people for reading earlier drafts of this paper and providing comments, suggestions, and criticisms: Gary L. Faulkner, Diane L. Miller, Cecil L. Willis, and *TAS* anonymous readers. Address all communications to: Robert K. Miller, Jr., Dept. of Sociology and Anthropology, Univ. of North Carolina, Wilmington, NC 28401.

"basic conflict between research and teaching—that to do one well precludes doing the other well, and that the latter activity is in some way morally superior" (176) is as widely accepted as Lewis implies. Among those who do, in fact, accept this argument, is it an article of *faith*, or it is accepted reluctantly by *some* as an article of *experience*? Do academics *typically* construct self-serving, self-deluding "excuses" for not doing and publishing research? Lewis informs us that many academics "have persuaded themselves—and each other—that there is an intense pressure to keep them constantly overworked" (176). This statement is unsubstantiated and gratuitous. Further, "publication is a necessary . . . condition for a successful academic career" *not* simply to the degree that the question-begging "Thomas Theorem" is in force. Research publishing has more to do with how seriously one takes the pledge of scholarship implied by acceptance of a Ph.D. and an academic position, as well as academic market forces, institutional rank, and other structural factors, which are only imperfectly reflected in "definitions of the situation."

The data Lewis presents on numbers of publications and their distribution are not so much evidence of low research productivity as of his evaluative position. And the data he reports from Fulton and Trow's (1974) study do *not* "mean that only half of the faculty are even prepared to publish" (177). Further, as a set, the data he cites do not support his contention that "more persons seem not to be publishing than are perishing" (177). He shows that more are *not* publishing than *are* publishing, but includes little or no evidence on the incidence of "perishing." I have none to offer here either. However, if the proportions of academics who are "fired" (not granted tenure) and who spend extended periods within academic rank before promotion are both increasing, while research productivity has remained fairly constant, then the conclusion that the academic's position is deteriorating is justified.

I agree with Lewis that negative caricatures and stereotypes are drawn of the "researcher," that they are clearly unfortunate, and that they may be motivated in part by "jealousy." However, criticisms of those academics who "merely" teach are probably equally common, vicious, and overdrawn. Both types are apparently counterproductive, and I suspect that their self-serving characters partially reflect academic market conditions.

My major disagreement with Lewis is in his implication that little or no publishing is evidence of lack of ability, intelligence, effort, or motivation. Clearly, "individual" factors such

as these are involved in the explanation of research productivity. However, I think Lewis exaggerates their explanatory power and makes no effort in his paper to explain how they are themselves "channeled" or affected by macro- and microstructural contexts. In other words, his accounting minimizes the importance of antecedent and intervening variables that are clearly relevant to any explanation of variations in research productivity. There are real and obvious differences in research and publishing opportunities among academic positions. Those in less "favored" positions may have very little published work to show for their research efforts. Further, not all varieties of research stand an equal chance of publication. Availability of journal space, as well as theoretical, methodological, and substantive preferences work to the advantage of some but to the disadvantage of others. I simply suggest that differential opportunity and a system not as "open" as implied by Lewis somehow operate in conjunction with "individual ability" to affect research productivity. No implication of unseemly "conspiracy" is intended.

Lewis concludes his article by paraphrasing Marx and Engels (180). I find this ironic, because the general thrust of his argument is social Darwinist and structural-functionalist. His assumption of a system operating on pure contest or achievement mobility, his argument for differential functional importance of academic tasks and differential rewards to insure completion of the most important tasks, and his exaggeration of the importance of differences in individual ability in explaining variations in research productivity are several examples. He suggests that "if publishing were highly valued, more people would probably do more of it" (176). If earning lots of money were highly valued, would more people probably do more of it?

In sum, Lewis's argumentative tone and evidential weaknesses detract from the value of his work. *The American Sociologist* is a forum for issues such as those addressed by Lewis. However, his essay leaves us essentially where we started. The causal structure which accounts for variation in research productivity remains to be established. It strikes me, therefore, that efforts to continue development of a sociological explanation of professional productivity would be of more lasting value. The research that could resolve our disagreement is inconclusive. However, my "sociologic" suggests that variance in research productivity cannot be adequately explained without explicit consideration of the effects of a set of social structural and organizational factors, in-

cluding sponsorship and publication networks, and departmental and institutional supports for research. Hopefully, this brief exchange will stimulate further research in this direction.

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OPEN ACCESS TO PUBLICATION*

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I commend the Editor of *The American Sociologist* for the way this exchange between Mr. Miller and me has been handled. Readers should understand the process. Mr. Miller wrote a rebuttal to my essay (Lewis, 1977). Two editorial referees, including myself, did not believe that it merited publication. Two other readers thought that it should be printed. Mr. Miller has revised his opinions in light of the four sets of comments. My remarks are thus a rejoinder to his somewhat reformulated critique.

Because editorial policy and competition for space dictate strict limits on the length of this rejoinder, it is not possible to respond to Mr. Miller's observations quibble-by-quibble. So that my remarks might have the right balance, I will move directly to his "major disagreement," namely, that "antecedent and intervening variables" have a significant effect on research productivity. This point is, of course, true, and many involved in research in the *Sociology of Higher Education* have addressed themselves to the question. For example, I have considered it elsewhere and after reviewing the recruitment process in universities, was sufficiently convinced of it to conclude: "Needless to say, the successful completion of one's research, particularly in the sciences, depends on adequate funding. All of this suggests an inescapable maze: who one is determines where one is, which determines how one's work is received, which determines where one is. It also suggests countless opportunities for some and dead-end careers for others" (Lewis, 1975:123). Moreover, in the final paragraph of the same chapter, the point is again made that "the system is less open than is generally acknowledged, and as a con-

sequence the access of some individuals 'to the means of scientific [and scholarly] production' is severely limited. In the most simple terms, what is operating here can be called the Matthew effect" (Lewis, 1975:146). Accordingly, Mr. Miller's main point is one that has been made many times, and can be readily found in academic journals and elsewhere.

It would seem then that those who insist that merit has more than a moderate influence on who gets ahead within an institution (promotion, recognition of merit, and the like) or in the discipline (publication opportunities, funding of research, editorial responsibilities, recognition by professional associations) are conveniently ignorant or disingenuous. Happily, Mr. Miller understands that the "system [is] not . . . 'open,' " but in stating that "the causal structure which accounts for variation in research productivity remains to be established" (p. 178) he reveals that it was all only a good guess. This assertion, like so much else in his critique (e.g., his steadfast belief that the principle of publish or perish operates to any significant degree in academic organizations), reveals such an abysmal ignorance of how institutions of higher learning function that it is a scholarly courtesy to suggest that he take the trouble to read on a subject before he sets his pen to paper. He then might realize that it is those, like himself, who maintain that academics must publish or perish who are making the "assumption of a system operating on pure contest or achievement mobility" (178)—not those who call such banalities in question.

Thus, in spite of Mr. Miller's contention that we disagree on this important and interesting point, it turns out, to my great relief, that this is not the case. Still it is unclear why he would even raise the subject in commenting on "Writers of the Academy, Unite!", an essay which focuses on research productivity within institutions and not within the discipline as a whole. My concern is with variation within departments where presumed individuals have similar "research and publishing opportunities." To be sure, those ascribed academic characteristics which individuals bring with them to a department may differentially affect their relative standing with their colleagues (a recipient of a degree from Harvard may generally be treated more favorably than a graduate of Michigan State), but for the most part such factors have not been shown to be of more consequence in holding a position or in advancement within a department than they are in initial placement, publication opportunities, and all the rest.

In comparing academics who work under similar conditions, one might want to talk

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about differential opportunity, e.g., this year theorists are having greater difficulty getting their work in print (or is it less difficulty?), but more of the variance in rates of publication can be explained by "ability, intelligence, effort, [and] motivation." Mr. Miller's invocation of such awesome sociological explanations of behavior as "macro- and microstructural contexts" only obscures matters, and too hastily lets pass the possibility that individual abilities and inabilities and responsibilities may have some explanatory value.

The intent of "Writers of the Academy, Unite!" is to direct attention to the ambivalence and skepticism about publication in academia. Mr. Miller says that "the scattered evidence presented by Lewis does not adequately support several of his contentions" (177). Yet, he does not offer a single example of contradictory interpretation of my statistical evidence. (Admittedly, there is an alternative reading of his ambiguous sentence, but it is unlikely that Mr. Miller would expect every statement in an interpretive essay to have documentation.) Miller may not be convinced by my argument, but perhaps if he were to consider his own words, and the evidence he offers, he would better appreciate how deep-rooted the problem, in fact is.

In the first place, the fact that he would even think, given his admittedly limited knowledge on the subject, of raising the question, that he would do so in such an obstreperous manner, and that he would then lean so heavily on "academic position, as well as academic market forces, institutional rank" (178) in his explanation of the place of publication in academic careers would seem to affirm my position. He pays lip service to differential ability but rejects the possibility that it might be the key to understanding differential rates of publication. Would he contend, with the spectre he raises of "sponsorship and publication networks," that those who publish are mostly able to do so because of their friends and other surreptitious means? Or might it simply be that what they have to contribute is more valuable or that they are more able and industrious than those who do not share ideas in refereed journals?

He also asserts that probably not very many people believe that there is a conflict between research and teaching, and that those who do have come to the conclusion "reluctantly . . . as an article of *experience*" (178). Is it because they have seen too many publishers shirking their teaching responsibilities? Could it ever be because they have been unable to get something on paper or find a publication outlet for their written work, and they are venting some

frustration? In other words, is this another way to belittle what to them is a rate-buster? Or is this out of the question, for who could believe that accusation, after all, that "academics *typically* construct self-serving, self-deluding 'excuses' for not doing and publishing research?" (178).

In sum, the phenomenon of informal networks is not as important as the fact that one clique does not control all of the publication outlets. There is enough diversity in any discipline to permit quality ideas to get a hearing. That is why we find "intellectual remnants seeking their level." That is why controversial theses such as those of a Velikovsky find their way into print. If your paper is not the sort that conforms with the biases of the *American Sociological Review*, try *Theory and Society*; if it is too conventional for the *Insurgent Sociologist*, try *Social Problems*. As their critics have pointed out, there are many more outlets that a sociologist can utilize besides the 22 volumes listed by Glenn and Villemez (1970). Favoritism, ascription, the old boy network, and the like merely reduce the vast number of pages that are open for competition; there is no evidence that they necessarily narrow the range of ideas that find their way into journals.

Quite simply, there is no problem with journals publishing poor quality material as long as it does not take up so much space that there is no room left for useful scholarly contributions. No one has ever shown that this is in fact the case, so we must assume that unpublished work is of lesser quality, maybe not than everything or even most of what is published, but certainly than anything useful that is published. The fact that many mediocrities are successful does not mean that more able people are prevented from succeeding. To even hold the suspicion that the bad has driven out the good is simply crazy. To draw an analogy between the activities of publishing and the process of distributing social rewards is less clever than indicative of the querulousness that marks Mr. Miller's commentary.

Like Miller, too many wring their hands about unfair advantage for a few, but when an argument is presented that draws the rather obvious conclusion that the most justice and progress result from a merit system they immediately raise the cry of elitism. One wonders which side they are on, and whether they would be such strong advocates of equity if they were the beneficiaries of preference and privilege.

Some of my work in the Sociology of Higher Education has led to the obvious conclusion that the level of scholarship in the contemporary university is not what it might be; too few

of the professoriate seem committed to the life of the mind. I find the poor quality of Mr. Miller's criticisms of "Writers of the Academy, Unite!" to be a disheartening confirmation of this generalization.

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LETTER

To the Editor:

I recently submitted to *The American Sociologist* an article entitled "Buddhist Sociology: Some Thoughts on the Convergence of Sociology and the Eastern Paths of Liberation." The article discussed basic assumptions of sociology, methodology, and the organizational structure and practice of our profession, in the light of insights from Eastern Thought. Revisions were suggested; the paper was revised and resubmitted. It was ultimately turned down.

At the risk of seeming a "poor loser," I would like to comment on the procedures and attitudes involved in this decision, as I feel that these imply a dangerous lack of openness to new ideas. On balance, three readers felt the work should be published. One reader was totally opposed. On this basis, the piece was refused. I would suggest that if agreement among four readers and the editor is required for publication, little that is iconoclastic or annoying to established professionals will ever be published in this journal.

Feelings against the article apparently ran very high. The reader who opposed publication responded to my plea for detachment of the Eastern sort as follows: "such disengagement strikes me as being *horrendously criminal, mad, malevolent*, to ourselves, to each other, to children yet unborn" (emphasis in original). Interestingly enough, the paper has in it a very heavy indictment of sociological practice and contains suggestions for very radical deep-going reform. But this reader apparently came to the manuscript with the usual Western misjudgment of Eastern thought: that it is a prescription for fatalism. It seemed to me that the reader couldn't "see" those parts of the

paper which ran directly counter to his or her previous assumption. The editor of this journal claimed he felt very ambivalent about publication, and he commented at length on his feelings toward issues raised in the paper. It seems to me that where such strong feelings are aroused, publication is indicated—unless we simply don't want to read anything that arouses strong feeling.

Two comments made by other readers trouble me also. One reader found the piece "arrogant," and another questioned its "soundness." First, it seems to me that calling an intellectual work "arrogant" implies that there are established truths which must be approached with reverence. Second, the term "sound" smacks dangerously of a mentality which asks people to stick by the established formulas of our field.

My colleague, Glenn Goodwin, who helped me greatly in the writing of the essay, wishes to sign this letter with me, and I am happy for his support. We feel that the procedures and attitudes involved in the decision on this paper go a long way toward explaining the sameness and distressing lack of original or iconoclastic thought which finds its way into established sociological journals. Have we become the "experts" of whom Krishnamurti speaks, who know so much that they cannot learn anything new?

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SPECIMEN ANNOUNCEMENT—NOT A REQUEST FOR APPLICATIONS

RESEARCH GRANTS IN HUMAN ECOLOGY

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Grants are ordinarily made to an applicant through the university or other non-profit institution with which he is associated. In exceptional cases, grants may be awarded directly to individuals when, in the judgment of the FUND's Board of Directors and advisers, such an arrangement is mutually desirable.

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- A concise statement of the problem and its significance.
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- A brief statement of procedures and methodology, with emphasis upon proposed data reduction and analysis techniques.
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To facilitate review, at least four copies of the application should be submitted. Research proposals are reviewed by the staff and by consultants who serve as advisers to the FUND's Board of Directors. Each reviewer makes independent recommendations regarding the scientific merit of the project. In the final evaluation of the proposal, the Board of Directors considers these recommendations as well as how the proposed research fits into the general program of the FUND and the availability of funds to support the project. Several weeks may be required to reach a decision with respect to any given grant proposal.

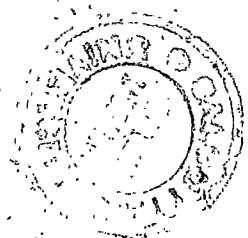
Investigators are welcome to confer in person or by mail with the staff of the HUMAN ECOLOGY FUND for advice and assistance in preparing a proposal or in initiating and carrying out a research task.

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The American Sociologist

Volume 13 Number 4 November 1978



An official journal of the American Sociological Association

EDITOR'S PAGE

While sociology has not yet felt the "crunch" of a surplus of PhDs as much as other fields have, particularly the humanities but also some of the sciences, we are all aware that it is increasingly difficult to place new degree holders and that the fat years of easy mobility and "guaranteed" tenure are over—and that we face at least seven lean years. (I use the term "surplus" because it is current; my personal view is that the notion of an oversupply of educated people is an anomaly.) There are, as we all know, a complex set of reasons for the current situation. We also know that there are a number of proposed solutions, such as institution of quotas, shared appointments, early retirement, abolition of tenure and expansion of employment outside of the academy.

The realities of educational finance and of market constraints are already producing sharp changes in prospective career patterns for many of our colleagues. Increasingly large numbers of sociologists, unable to find regular academic positions, are becoming part-time teachers or academic transients, moving from one visiting position or "soft money" job to another. Our first feature in this issue consists of two articles on the part-time phenomenon: Tuckman, Caldwell and Vogler give us a sketch of the aggregate picture, and Van Arsdale a personal memoir by a "victim" of the system (Van Arsdale is not a sociologist—I have been assured that his description is a valid characterization of the situation of many sociologists). The articles are followed by comments and rejoinders.

A widely endorsed solution to the problem of relative decline in academic job opportunities is to more effectively integrate sociologists and other social scientists into nonacademic sectors. Proponents of this position suggest that if we were to slightly change our graduate training programs and simultaneously demonstrate to nonacademic employers what social scientists could do for them, employment difficulties would be sharply reduced. This solution has also been suggested for anthropology. Our second feature consists of a sharp attack by Paul Kay, an anthropologist, on both the practical utility and the morality of this proposal, again with comments and a rejoinder. Those interested in this debate may want to look at the *Anthropology Newsletter* where Kay's statement was originally published—and where his colleagues are responding.

Lyson and Squires present another view of the employment picture, in this instance a report on indignities suffered by some sociologists seeking academic employment. While recognizing that we are currently in a "buyer's" market, they argue that

civility and courtesy should still be a part of collegial interaction, and suggest several ways in which the painfulness of job searches might be somewhat reduced.

Given that our first two features and the Lyson and Squires paper are posited on an assumption that there will be too many sociologists, the third feature many seem somewhat anomalous. Stebbins suggests that we should recruit amateur sociologists, as has been done in other sciences and in some of the humanities, both to heighten public support of the intellectual enterprise and to assist in data collection and analysis. We have again sought comments and provided Stebbins with an opportunity to respond.

* * * *

The issue closes with a comment on an earlier paper. Buttell addresses Catton and Dunlap's paper on the New Environmental Paradigm, which appeared in our special issue on "alternative theoretical perspectives" (TAS 13, February 1978). Catton and Dunlap have responded to Buttell.

Several features for our next volume year are well along toward completion. Our February issue will include exchanges on the nature of sociological knowledge and on how some of that knowledge gets reported in the ASR. A later issue will contain a major feature on freedom in teaching, research and publication, with reports from a number of different countries. Other projects are in the planning stages; we continue to welcome your suggestions and particularly your submissions on topics of general professional concern.

Finally, it is once again my pleasure to acknowledge the critical contribution of a large number of "special" readers who have read manuscripts since our last listing. Some of these reviewers have been true rate-busters in the reviewing process, many have played important roles in shaping special features, and as before, a number have brought perspectives from other disciplines to their reviewing. Thanks are due also to the large number of our colleagues, again from several disciplines, who have contributed comments for exchanges and debates in this volume. I have learned from all of these people, as I have from members of the editorial board and from those with whom I work in Bloomington. Many an otherwise dismal day has been brightened by a thoughtful review, particularly trenchant comments, a letter with ideas for the journal or expressing support for our several projects. Many of our colleagues, as a bonus, are witty as well as wise! I thank you all.

A processing fee of \$10 is required for each paper submitted; such fees to be waived for student members of ASA. This reflects a policy of the ASA Council and Committee on Publications affecting all ASA journals. It is a reluctant response to the rapidly accelerating costs of manuscript processing. A check or money order, made payable to the American Sociological Association, should accompany each submission. The fee must be paid in order to initiate the processing of the manuscript.

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PART-TIMERS AND THE ACADEMIC LABOR MARKET OF THE EIGHTIES

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The American Sociologist 1978, Vol. 13 (November):184-195

Less well-known than some of the other trends in academe, the growth in the number of part-timers in the last few years has been dramatic. This paper examines the questions of who these part-timers are, what the reward structure is under which they operate, and what the problems are that their growing use is likely to create. Data are presented from a national sample of part-time faculty conducted in the Spring of 1976 and these are used to analyze some of the problems that part-timers currently face in academe. We strongly suggest that part-timers will be around the campus green for some time to come and that more carefully conceived policies to accommodate their intelligent use are needed.

Major changes have occurred in academe in the past few years with important implications for how the professoriate of the 1980s will be structured. Less benefaction from private donors and benefice from state legislatures, a decreased rate of growth in federal research funds, and higher purchasing prices resulting from the inflation of 1974 and the rising cost of energy have put a severe strain on academic budgets. Declining student enrollment in the late seventies and early eighties, and the shock waves from the passage in California of the Jarvis-Gann proposal (also called Proposition 13) are both likely to further intensify the financial pressures on academic institutions.

Tight budgets have affected the academic labor market in several ways. An increasing number of continuing faculty are receiving salary increases well below those of the 1960s, some new assistant professors are being hired at salaries similar to those received by their peers two or three years before, and in some states sal-

ary freezes have occurred in one or more years. Concomitantly, the number of new assistant professor positions has declined precipitously in fields with shrinking enrollments, the queue of young persons desiring an academic position and unable to attain one has increased, and the chances for academics to move upward or even laterally have diminished. These changes have been well chronicled, but their implications both for the quality of knowledge transmitted to future generations and for those pursuing an academic career have not been well explored.

Academic institutions have tried a number of ways of reducing instructional costs. Some have resorted to the time-tested approach of raising student-faculty ratios, arguing that the evidence suggests this has no significant effect on learning. This has reduced the demand for new faculty, in some instances decreased the number of faculty given tenure, and increased teaching loads. Other institutions have explored the use of educational technology, employing computer assisted instruction or mass TV courses in an attempt to reduce instructional costs. While the evidence is not yet in on this approach, early studies suggest that technology usually becomes a supplement rather than a substitute for faculty time, and as a result, increases instructional costs.¹ Still other institutions have increased the number of part-timers on their faculties, tempted by the lower salaries and

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¹ That strong pressures exist which make it difficult to replace faculty with the new instructional media is discussed in some detail in Jamison et al. (1976:62-88).

fringe benefits and the increased flexibility that this type of instruction facilitates. A large and growing number of academic institutions now employ part-time teachers, and as a result, the part-timer is emerging from the shadows as a force to be reckoned with in the next ten years.

The Growing Use of Academic Part-Timers

Less well-known than some of the other trends in academe, the growth in the number of part-timers in the last few years has been dramatic. Between the 1972-73 and 1976-77 academic years alone, the National Center for Education Statistics (NCES) estimates that the number of part-timers increased by almost 50% while the number of full-timers increased by less than 9%.² These aggregate figures would seem to suggest that at least part of the tightening in the full-time labor markets during the early 1970s might be attributable to the growing use of part-timers. However, the situation differed radically among institutions. At the university level, approximately 5,100 new part-time positions were created during the four-year period, while the same number of full-time positions were eliminated. While the number of part-time positions increased relative to the total, part-timers remained a minority and the labor market exhibited signs of stagnation, if not retrenchment. In contrast, at the four-year institutions about 32,000 new full-time and 26,000 new part-time positions were created. The part-time population rose by 56%, compared to an increase of 19% in the number of full-timers. By the 1976-77 academic year, part-timers represented 27% of all persons employed at four-year schools. These figures suggest a fairly healthy growth in full-time employment at the four-year institutions, despite growing use of part-timers.

This was not true of full-time employment at the two-year institutions. In the 1972-73 academic year, there were about 48,040 part-timers and 73,490 full-timers employed at junior colleges, technical in-

stitutes, and other two-year schools. By the 1976-77 academic year, the respective employment figures were 86,680 and 81,790. As a result of an approximately 80% increase in part-time employment, part-timers rose from 40% to 51% of the total teachers employed at two-year institutions. It is quite likely that in the junior college markets, the shift toward part-time faculty significantly affected the demand for full-time faculty, perhaps to the detriment of new PhDs seeking full-time jobs at these institutions.

If present trends persist, by 1985 part-timers will represent between 38% and 45% of all persons employed in academe and between 70% and 80% of those employed at junior colleges. Should academic institutions decide to reduce even further the size of their full-time faculty, the above percentages could increase substantially. And while it is true that part-timers currently constitute less than 20% of the faculty at the universities, if the full-time population continues to shrink part-timers could comprise 30% to 35% of all the university faculty employed in 1985.

Given the likelihood that part-timers will be playing an increasingly important role in instructing students in the next decade, it is important for us to learn more about who these persons are, the reward structure within which they operate, and the problems that their growing use is likely to create. Before proceeding with this agenda, we will briefly discuss the data from which our subsequent analyses will be drawn.

The National Study of Part-Time Faculty

Under a grant from the Ford Foundation, the American Association of University Professors (AAUP) conducted a survey of part-timers employed at institutions of higher education in the Spring of 1976. The survey design involved two stages. In the first, a 5% stratified proportionate sample was drawn from the 2,892 institutions which the NCES reported had employed part-timers in the 1972-73 academic year.³ The stratification was by pri-

² These figures were obtained from the NCES (1976) and may tend to underrepresent occasional part-timers.

³ The NCES completed a second study of part-timers in 1976, well after our own study was in the

vate or public ownership, level of degree offering, region of the country, and number of full- and part-time faculty. Institutions selected by this procedure were then contacted and those willing to participate were sent questionnaires to be distributed to all part-timers then on their payrolls. Those that refused were replaced by other institutions as alike in characteristics as was possible. Part-timers were assured of the complete confidentiality of their responses and were requested to mail their completed questionnaires directly back to the AAUP central office. Of the approximately 10,000 questionnaires distributed, 3,763 or about 38% were returned from persons in 128 academic institutions. Further details on the sampling procedure can be found in the AAUP Bulletin (1978).

The final sample is probably somewhat overrepresentative of part-timers in junior colleges, and may underrepresent the occasional part-timer population. Given the extraordinary difficulties that academic institutions seem to have in identifying their part-time employees, and their reluctance to furnish data that might be used for collective bargaining or salary equalization purposes, we believe that the data reported below are as good as we can hope to obtain at this time. Moreover, comparison of our results with the limited number of other sources of aggregate data suggests that they are reasonably representative of the national population of part-timers.

Differentials in Compensation and the Question of Rank

A question that has drawn the interest of part-timers and full-timers alike is whether part-timers are paid equivalently to full-timers for the work they perform. It is not an easy one to answer. At present, no simple yardstick exists to compare the credentials of part-timers and full-timers and their respective workloads. The part-timer may bring fewer credentials and more practical experience to the job; the full-timer, a better background in theory and greater familiarity with the literature.

field. It showed that the number of part-timers had increased relatively at the two-year institutions and decreased relatively at the universities.

Time spent in preparation for teaching, in outside reading, in working with students, in research, and in other activities may differ. The faculty member's value to his or her employer may depend on a familiarity with the department's orientation, on the person's reputation, or on a host of difficult-to-quantify variables. Nonetheless, an answer must be found for at least three reasons. First, many part-timers (over 50% in our sample) feel they are paid proportionately less than their full-time counterparts. As more of these persons take their grievances before the courts or other public bodies, pressure will grow to provide better data about the legitimacy of their claim. Second, with collective bargaining on the rise and the unions competing for potential members, pressure to include part-timers as part of a bargaining unit is likely to increase. At the same time, the need to define an equitable pay scale for part- and full-timers is also likely to grow. Third, to the extent that academic institutions recognize that they can acquire part-timers at a cheaper salary than full-timers, at least some will substitute the former for the latter. As the proportion of part-timers increases, pressures from full-timers to eliminate this salary differential are also likely to increase.

A rough measure of the differential in salary between full-timers and part-timers was calculated by two of the authors in a recent paper reported in the AAUP Bulletin (Tuckman and Vogler, 1978a). Using salary data from the AAUP Bulletin (Dorfman, 1977) for full-timers at each rank and at the three types of institutions (two-year, four-year and universities), and data on full-time workloads and hours from a 1972 national study of faculty (Bayer, 1973), the authors provide wage rate equivalents for full-timers that can be compared to the wage rates of part-timers at the same types of institutions. Several different hours measures are used to compute wages. The first simply involves hours spent in the classroom or laboratory and corresponds to a student contact hour measure. The second is a measure of total work hours and adds to contact hours the time spent in counseling, supervising, administrative activities, and teaching-related activities. A third measure computed is based on courseload.

To render the part-time and full-time groups comparable, an assumption must be made about how to treat a part-timer's rank. This is because over 70% of the part-timers are in unranked positions, compared with only 12% of the full-timers. When comparisons are made based on the assumption that part-timers are correctly classified by rank (i.e., unranked), there is little evidence that they are underpaid. Indeed, their wage rates are roughly equivalent to full-timers in unranked positions according to all of the measures. However, if the comparison is made on the assumption that part-timers should have a rank structure equivalent to that of full-timers, it appears that part-timers are paid roughly 25% to 35% less than their full-time colleagues, depending on which wage measure is selected.

Our own view is that given the distribution of experience, degrees, and other personal characteristics observed in the data, a higher proportion of the part-timers should hold ranked positions. Viewed in these terms, our results suggest that, on average, the part-timers in our study are underpaid relative to full-timers with equivalent academic credentials. They also suggest that the part-timer ranks fairly low on the professional totem pole both in terms of prestige and academic salary. It is not clear how this colors the part-timer's view of academe but it is almost certain to have had some effect.

Differentials in Compensation and the Question of Fringe Coverage

Different treatment of part- and full-timers in academe is even more obvious in the area of fringe benefit coverage. In recognition of the fact that some institutions distinguish for fringe benefit purposes those part-timers employed half-the-load or less of a full-timer (the "lessees") from those employed more than half-the-load (the "mores"), we have disaggregated part-timers into two groups.⁴

⁴ Part-timers' hours are obtained from the AAUP survey questionnaire. Full-time hours are obtained from the ACE data (Bayer, 1973). A matrix of full-time hours is then prepared using discipline, public or private control, and level of degree offering to disaggregate the ACE data and a computation of

The results are revealing. Almost all full-timers are covered by social security or another retirement plan. In contrast, only about 40% of the "lessees" and about half of the "mores" receive retirement coverage from their academic jobs. Over 85% of all full-timers receive workmen's compensation coverage; a little more than 9% of the "lessees" and about 19% of the "mores" are covered. And while over 95% of the full-timers are covered by some form of medical insurance, less than 5% of the "lessees" and less than 21% of the "mores" are covered.

This difference highlights another way in which full-timers receive higher compensation than part-timers, and thus another incentive for employers to hire part-timers rather than full-timers. The more limited coverage of the part-time group, and the lower labor costs this implies, are a vestige of the time when part-timers were a comparatively small group. They probably also reflect the ambiguous or nonexistent coverage of the part-time group under many state and federal laws. Faculty and institutional personnel committees that formulate fringe policies have largely ignored the fringe benefit needs of part-timers, many of whom now rely on academic employment as a sole source of fringe benefit coverage. This lack of coverage is likely to become a major source of concern in the coming decade.

Are Part-Timer and Full-Timer Salary Differentials Determined by the Same Factors?

An abundant literature suggests that the salaries of full-timers are determined partially by their experience, by geographic location, by the discipline in which they teach, and by such personal characteristics as when they start their careers and how much they publish (Tuckman, 1976). If part-timers were paid proportionately to their full-time counterparts, we would expect their salaries to be determined by the same factors.

There are several reasons for believing that part-time salaries are established in a

each part-timer's percentage of a full-time workload is made for both contact and total hours. This allows us to assign persons to the two categories.

different marketplace than those of full-timers. In some geographic areas part-timers are hired by either one or very few employers. The supply of potential part-timers is usually fairly fixed since a majority of part-timers either have a second job or have a preference for staying in the same geographic area. The supply of part-timers is probably larger than that of full-timers, both because academic employers can draw on persons with full-time jobs to teach an occasional evening or off-hours course, and because part-timers can be hired with a lesser degree or with less experience than full-timers. Separation of the two markets is further assured by the existence of asymmetric mobility: full-timers can teach part-time far more easily than part-timers can teach full-time. As a result, it is possible for employers to pay part-timers lower salaries and to create a different reward structure than would exist in a single market for part- and full-time faculty.

The above arguments suggest that the salaries of part-timers are likely to be determined by different variables than those of full-timers; they do not suggest which variables will differ. In a recent paper (Tuckman and Caldwell, 1978), two of the authors provide several insights into this question using a linear regression model with the Spring 1976 earnings of part-timers as the dependent variable and independent variables derived from a regression model previously used to examine full-timers' salaries (Tuckman, 1976). Separate equations were run for two-year institutions, four-year institutions and universities. Several findings are of particular interest:

1. Race and marital status, important explanatory variables for full-timers, are not statistically significant in the salary equations for part-timers.

2. The various field variables are significant only in the university equation.

3. Part-timers with doctorates receive significantly higher salaries than those without only at four-year institutions.

4. Part-timers who indicate a willingness to move do not have higher salaries than those unwilling to move.

5. Females earn somewhat more than males.

6. Part-timers at public two-year institu-

tions earn least, followed by those at private institutions; those at four-year institutions and universities earn the most.

7. An incremental teaching hour is worth \$118 to part-timers at two-year institutions, \$165 at four-year institutions, and only \$74 at universities.

8. Rank is important as a salary determinant at the universities and four-year schools. It is responsible for large differences in salaries only at the universities.

9. Activities such as administration, teaching and publication are significant determinants of salary differentials only at four-year institutions. This is surprising since in our full-time study, the universities were the most likely to reward these types of activities. It may be because most part-timers are used largely in the basic courses.

10. An additional year of experience does not produce a statistically significant increase in part-timer salary except at the junior colleges, where an additional year of part-time experience adds about \$25 to Spring 1976 salary. Although four alternative measures of experience are employed, our conclusions remain unaltered by different specifications of the experience variable. (Tuckman and Caldwell, 1978:19-22)

(Average Spring 1976 salaries were \$1,165 at two-year institutions, \$1,950 at four-year institutions and \$2,691 at universities.)

These findings support the argument that the variables that explain differences in salaries among full-timers are not the same as those that explain differences among part-timers. For full-timers, personal differences involving both educational level and proven skills have a direct effect on salaries; for part-timers they do not. What is perhaps even more important is that with few exceptions, institutional factors are the major determinants of salary variation for part-timers. This is consistent with the view that the buyer of part-time labor rather than the seller is the primary determiner of the terms of the employment contracts.

The absence of a statistically significant salary increment for experience is also important. Since full-timers receive this increment while part-timers do not, those

part-timers who retain a part-time position are likely to fall behind their full-time counterparts over time, *even* if they were initially hired at the same salary level. They are also likely to find their real incomes declining through time. To the extent that this conclusion is valid, it is likely that long-term part-timers familiar with the salaries of full-timers in their employing departments will be dissatisfied with their situations.⁵

A Taxonomy of Academic Part-Timers

Persons have different reasons for accepting a part-time position and no single statement can adequately describe either their behavioral objectives or their reactions to the part-time environment. Thus, it is useful as part of our analysis of part-timers in academe to distinguish among several conceptually distinct groups. Clearly, a number of different taxonomies can be developed based on age, income, life cycle or other distinctions. Our primary concern is to distinguish among part-timers in terms of their labor supply behavior, and of how part-time employment fits into their long-term career objectives. Thus we have divided the AAUP data into seven mutually exclusive categories based upon the different reasons why the persons in our study reported choosing part-time employment. An hierarchical ordering is employed to ensure that a person falls into only one category. Thus, a person placed into the first classification cannot fall into the second, etc. This is done to ensure that the categories are independent and that the persons in the sample are disaggregated into different groups. It enables us to examine whether the groups differ in terms of labor supply response, attitudes, and socioeconomic conditions (Tuckman, 1978). Since the way in which the categories are ordered makes a difference in terms of which category a part-timer is placed in, considerable thought and experimentation preceded the final ordering of the categories.

⁵ As economists, we have chosen to avoid dealing with the issue of career satisfaction because of our lack of expertise in this area. No doubt this is an issue of some importance to the part-timer, and it merits further attention from those trained in the field.

Semi-Retired—those reporting their primary reason for becoming part-time is that they are semi-retired (2.8% of the sample). *Students*—persons employed in other departments than the one in which they are registered to receive a degree and who are called part-timers rather than graduate students by the institutions that hire them. To be a student, the person must report that he or she is currently registered for a degree (21.2%).

Hopeful Full-Timers—persons who report that their primary reason for becoming part-time is that they couldn't find a full-time position (16.6%).

Full-Mooners—persons who in addition to their part-time job held a full-time job of 35 hours a week or more for 18 weeks or more (27.6%).

Homeworkers—persons who report that their primary reason for becoming part-time is to take care of a relative or child (6.4%).

Part-Mooners—persons holding two or more part-time jobs of less than 35 hours a week for more than one week (13.6%).

Part-Unknowners—persons whose motives for becoming part-time do not fall into any of the other categories (11.8%).

The percentages reported in parentheses reveal several interesting things about the part-timers in the sample. Surprisingly, Homeworkers constitute a small proportion of all part-timers, dwarfed by the Students and Full-Mooners, who together comprise almost one-half of the sample. The number of hours that persons in both of these groups are willing to offer as academic part-timers is likely to be limited, as are their loyalties to the institutions that hire them. Hopeful Full-Timers and Part-Mooners also constitute large groups. We suspect that the size of the Hopeful Full-Time group is related to the tightness of the full-time academic labor market and that it will grow larger in both relative and absolute terms if student enrollments decline. Finally, the Semi-Retireds and Part-Unknowners constitute a fairly small percentage of all part-timers.

Part-Timers' "Other" Jobs

Given the fact that a large number of part-timers have one or more other jobs, it is useful to understand more about what these jobs are and how much part-timers earn from them. Table 1 shows the aver-

TABLE 1
INDUSTRY DISTRIBUTION AND AVERAGE HOURLY WAGE FOR ACADEMIC PART-TIMERS
WITH SECOND JOBS, SPRING, 1977

Industry	Percentage with Full-time (additional) Jobs in this Industry*	Percentage with Part-time (additional) Jobs in this Industry*	Average Hourly Wage**
Commerce	11.6	6.4	\$14.76
Manufacturing & Construction	7.7	3.4	15.42
Retail or Wholesale Trade	3.1	3.3	11.85
Transportation or Public Utilities	1.4	0.6	13.34
Agriculture or Mining	1.4	0.6	11.06
College or University	18.0	26.3	16.43
Technical Institute	1.8	1.6	13.52
Professional School	0.7	1.7	16.18
Elementary or Secondary School	16.6	11.9	13.41
Other Business	5.1	6.6	13.36
Human Services	5.6	4.9	12.43
Military Service	1.2	1.5	18.75
Government	9.6	3.8	13.58
Medical or Health	3.7	5.0	14.99
Other	12.6	22.4	15.36
Total	100.0%	100.0%	14.69

* These categories are mutually exclusive.

** Part-timers average hourly wage, computed by aggregating income from all other employment and dividing by total self-reported weekly hours.

age wages and distribution of part-timers' second jobs.

These data suggest that the major second job of part-timers, whether part- or full-time, is education, and that earnings from this source are relatively low. This is not surprising since even before the sudden increase in the use of part-timers, academic institutions relied on their full-timers to teach an occasional extra course on a part-time basis or called upon teachers from another institution or a public school to fill in during the evening. What is interesting is the fact that of the approximately 40% of part-timers with second jobs in the education field, more than half are at other colleges and universities. Apparently these persons are subject to the same pressures on their salaries from shrinking enrollments and tighter academic budgets in their second job as they are in their first. Perhaps somewhat more fortunate are the 26% of the working part-timers in the business fields. These persons earn higher salaries than most of the other part-timers and appear to be affected by a different set of fringe and wage policies.

Part-Timers Without Other Employment

Approximately 30% of the part-timers in the AAUP sample have no employment other than a part-time job in academe. As might be expected, these tend to be persons who teach more than half the load of a full-timer. By far, the two groups most likely to fall into this category are the Hopeful Full-Timers and the Homeworkers. In terms of wages received these groups are somewhat similar, but their household incomes differ substantially. This is because Homeworkers are more likely to be married (97%), to be female (97%), to have a working spouse, and to have household incomes in excess of \$15,000. Hopeful Full-Timers are more likely to be male (47%), somewhat less likely to be married (78.0%), and are less likely than the Homeworkers to have household incomes above \$15,000.

Of all of the groups identified earlier, the Hopeful Full-Timers are the ones most likely to be sensitive to the inequity of the salaries and fringes they receive. Originally attracted to a part-time position either to temporarily give up a full-time

career, to acquire teaching experience, or because they could not find a full-time job in academe, these people find it difficult to move to a full-time academic position. Many eventually exit academe to find full-time work elsewhere or assume two or more part-time jobs, which usually do not provide the same income and fringe benefit coverage as would a full-time job.

About half of the Hopeful Full-Timers working a full-time load are not included under a retirement plan, and over three-quarters are unable to obtain life insurance coverage, unemployment insurance, workmen's compensation, or sick leave. A majority of these persons also feel that they are paid less than proportionately to full-time equivalent faculty within the departments that employ them and most do not have a vote at faculty meetings.

It is difficult to avoid the conclusion that a barrier separates these part-timers from their full-time counterparts, differentiating not only their income and fringe coverage but also their rights and responsibilities within their employing institutions. For many persons within this category, part-time employment is a poor substitute for the full-time position they would prefer. Career progression proceeds, if at all, in a disorderly rather than an orderly fashion.

For the other categories of part-timers who have no other employment the picture is less clear. Many, if not most, Semi-Retireds are likely to value part-time employment as an orderly step toward retirement. For some of these persons, uneven fringe coverage or a salary lower than a full-timer's may be less important than the opportunity to remain in the labor force, albeit on a part-time basis. However, only about 44% of the Semi-Retireds receive retirement coverage on the job and less than 30% receive any type of health benefit coverage. This would be a source of concern for some part-timers in this group. Likewise, while many Homeworkers may be pleased with the opportunity that part-time employment gives them to earn extra money for the household, to get out of the house for a portion of each workweek, or to retain an interest in academe, some of these persons will not be pleased with the lack of opportunities to gain retirement coverage

independent from their spouses' earnings. Moreover, the absence of life insurance and health benefit coverage will, no doubt, be of concern in those families where fringe coverage is not available on the spouse's job or where it is costly to insure the entire family. Finally, the rise in women's awareness groups, as well as a growing sense of independence among women, is almost certain to increase interest in the adequacy of fringe coverage among the Homeworker group.

Student part-timers are probably somewhat more likely than those in the other categories to be satisfied with a part-time position. With the number of assistantships and fellowships shrinking at many universities, part-time employment in academe now provides a major means by which students can finance their graduate training. Conditioned by graduate schools not to expect retirement, life insurance, and medical insurance as teaching assistants, and probably less aware than some other part-timers of how important this coverage can be, the students tend to be a more accepting group. Even the lower rank or salary (relative to full-timers) can be tolerated since these inequities are assumed to disappear after the students get their degrees. For students, a part-time job is likely to be viewed as temporary, and short-term dissatisfactions can be accepted with the knowledge that they must be borne only temporarily.

This discussion suggests that part-timers are not alike in their needs or in the extent to which they have a stake in part-time employment. In the discussion which follows we shall refer to some of the key issues likely to arise out of the growing use of part-timers during the rest of this decade and into the 1980s. It should be clear, however, that these policies do not necessarily have the same importance to, or the same impact on, the different categories of part-timers.

The Issue of Proportional Income and Fringes

With widespread interest in equity issues in academe, the question of how to provide equitable remuneration to part-timers is likely to gain importance in the

next few years. Repeated analyses of our data suggest that part-timers are hired at lower ranks than full-timers with similar credentials, that they are not rewarded financially in a similar manner for service to their employing institution, and that they are rarely promoted or offered a full-time position (Tuckman, Caldwell and Vogler, 1978). In a few cases academic institutions have established a defined career ladder for part-timers: For the most part, however, career progression does not exist and part-timers are paid under a policy that mandates a flat payment per course, student hour, or class hour—a payment that bears little relationship to full-time salary rates.

Proportionate payment of part-timers would require that part-timers be paid an amount equal to what a full-timer with similar personal characteristics and a similar workload would receive for performing the same activities. Establishing appropriate criteria for comparing part- and full-timers would be difficult, but not impossible; the growing list of successfully resolved affirmative action cases provides ample proof that this can be done when it has to be. There are, however, two unique aspects of this situation. First, the full-timer is often called upon to perform certain tasks whose monetary value is not easy to measure. These include college-wide committee work, student counseling, etc. Full-timers must be granted a salary differential in recognition of these other activities. If they are not, then an incentive exists for them to ignore these activities or to file a grievance on the grounds that their efforts are unrewarded. Unfortunately, serious problems are involved in pricing these activities and a solution is likely to increase both paperwork and costs of administration in those institutions where it is attempted. Second, while some fringe benefits are easy to prorate, others are not. An equitable solution would seem to require equal access to such fringes as medical or dental services and the proration of part-timers' contributions along workload lines (see Tuckman and Vogler, 1978b:18-23). At a minimum, a workload determination would have to be made by the employing institution and specific guidelines laid down about which fringes a part-timer

would be eligible to receive (Catalyst, 1973).

Measures such as these would have a major effect on the intra-institutional differential between part-time and full-time salaries and would thus eliminate the incentive for institutions to hire more part-timers. In the absence of uniform national or even state-wide fringe standards, it will be difficult to solve the fringe problems of part-timers employed at several different institutions. Professional associations should provide a set of recommendations about which fringe benefits should be available to part-timers, how these benefits should be prorated in accordance with workload, and what options should be available to those with multiple jobs. Such guidelines would be advisory, of course, and would serve mainly as a model for those institutions interested in providing better fringe coverage to their part-timers. Such a model is long overdue and it could go a long way toward solving the problems of those with dual employment status.

Employment Security and Academic Freedom

The growing use of part-time employees is likely to pose a potential threat to academic freedom in the next decade for several reasons. Part-timers are usually employed for one term or less and very few have formal contracts. Under these circumstances employment security is limited and dismissal can occur without appeal. Part-timers also tend not to be represented by unions or professional associations and many are not covered under formal grievance mechanisms. This makes it difficult for them to gain a fair hearing at the institutional level and to marshal the support of their full-time colleagues. Moreover, given the limited number of academic employers in many areas, a part-timer dismissed by an employer may have difficulty finding another academic job. Thus, loss of one part-time academic job may not be followed up by employment on another.

These considerations can potentially lead some part-timers to avoid controversy in the classroom by, for example, choosing non-issue-oriented readings or topics of discussion, or not offering criti-

cal opinions. It is extremely difficult to evaluate the extent to which academic freedom might be threatened in this context. It may be that part-timers will find employment at institutions where the academic administrators encourage dissent. Alternatively, part-timers may find that by default their teaching goes unnoticed or unevaluated. However, should his or her views be singled out for attention, the average part-timer can count on little support in maintaining the part-time position. It seems likely that at least some part-timers would react to this implied threat by muting their criticisms.⁶

A limited number of academic institutions have solved this problem by allowing their regular part-timers to be eligible for tenure. By requiring that part-timers live up to the requirements imposed on full-timers, these institutions attempt to develop more uniform quality and a set of proven skills among their regular part-timers. While this practice eliminates the threat to academic freedom that the short-term contract implies, it is not likely to become widespread. Providing tenure to part-timers eliminates some of the major advantages of their use. Institutions lose the flexibility of hiring faculty when needed, of changing the ratio of faculty in the different disciplines, and of introducing new persons with different perspectives into the classroom. It is difficult to imagine that institutions with a dominant position in an area will voluntarily forgo these advantages. Ultimately, the issue boils down to the question of whose freedom, the employer's or the employee's, is to be the dominant one. The resolution of this issue will almost certainly involve a compromise between the two.

One way to accomplish this would be to introduce an informal seniority system in which part-timers and their professional associations would have a greater say over *who* will be hired, while the institutions retain their control over the *number* of persons hired. Such a procedure would provide a measure of job security by mak-

ing it more difficult for an institution to refuse to hire an employee without cause. Its ultimate success would, of course, depend upon the establishment of an acceptable mechanism for ensuring that the seniority system was observed.

Part-Timers and Departmental Governance

Another issue likely to require attention in the coming decade is the appropriate role of part-timers in matters of faculty governance. While a limited number of departments accept part-timers as partners in matters dealing with faculty governance, the vast majority do not. The data reveal that few part-timers have a vote at department meetings, few are consulted on curriculum matters, and few are asked for an opinion on course prerequisites, book adoptions, or related issues. An argument can be made that this is appropriate, since part-timers do not have the same stake in an institution as full-timers, are usually transitory, and lack the knowledge to deal with some or all of these matters. Indeed, full-timers sometimes express the fear that providing a voice to part-timers would lead to irresponsible decisions, which they would be left to deal with long after the part-timers had gone.

There are strong elements of truth in this argument but much depends upon the individual institutional setting. In two-year institutions, departmental decisions tend to require less familiarity with the subject, decision-making tends to be somewhat less democratic, and part-timers are rapidly becoming a majority of the faculty. Under these conditions, strong pressures will probably develop in favor of a role for part-timers in the decision-making process. The environment at four-year institutions is not as conducive to part-timer participation. In these institutions, part-timers are more likely to be regarded as adjuncts who should play a minor role in departmental affairs. In some cases, part-timers may alternate between full- and part-time positions and their rights will tend to be established on an ad hoc basis. However, given the teaching orientation of many of these schools and their emphasis on tradition, it

⁶ The importance of this problem may differ in the various labor markets. At the public universities, this is likely to be less of a problem, on average, than at private four-year schools. At present, no data exist to indicate whether this is an issue among part-timers.

does not seem likely that part-timers will be easily granted an input into the governance process. Finally, it is doubtful that many universities will allow part-timers an equal voice in the decision-making process. Faculty governance tends to be an important issue at these institutions and it is difficult to imagine that full-timers will give up their hegemony easily.

A finer distinction is likely to emerge between regular and occasional part-timers, with the former gaining somewhat greater representation within their departments while the latter do not. It is too early to tell whether this will take the form of equal or proportional representation, or whether it will involve the presence of a part-time representative. At a minimum, part-timers are likely to obtain better representation at faculty meetings.

Limits on the Use of Part-Timers

With the number of institutions employing part-timers on the rise, and the number of part-timers at two- and four-year institutions also increasing, it is quite probable that an attempt will be made to limit the number, if not declare a complete moratorium on the use of academic part-timers. Fueled by falling enrollments which threaten to shrink the number of full-time positions, by resistance to encroachment on traditional prerogatives, and by pressures on those disciplines facing major enrollment drops, such a movement is likely to be met with favor by some full-timers. Whether these persons will have enough market power to force such limits on their employers is doubtful. However, some inroads can be made on state legislatures and Boards of Trustees of institutions employing part-timers if it can be shown that their use affects the quality of education these institutions provide. In this case it would not be surprising to see the professional associations adopt more stringent criteria for the selection of part-time faculty.

Our own judgment is that the use of quotas or other mechanisms designed solely to limit the growth of the part-time population would be a mistake. A better approach would be to pay part-timers proportionately to their full-time counterparts, using a similar set of stan-

dards to judge the performance of each group. When the nonjustifiable salary and fringe differentials between the two groups are eliminated, the decision of whom to hire depends on largely noneconomic factors such as scheduling needs, the quality of instruction and the cohesion of the department. Other factors such as the administrative costs of hiring and supervising part-timers also assume greater significance. In those institutions where part-timers provide a distinct advantage over other faculty, hiring would continue; in institutions where the benefits they offer are marginal, new hirings would decrease. Our expectation is that the trend toward the greater use of part-timers would either be slowed down or reversed as institutions that hired part-timers solely to reduce instructional costs would no longer realize this saving.

Part-time faculty provide an important source of flexibility at a time when the options facing academic institutions are limited. Although some full-timers would prefer to see the part-timer disappear from the campus green, this is not likely to happen in the next decade. More likely will be a consolidation of the part-timer's gains at the universities, some growth at the four-year institutions, and a continued increase in the number of part-timers at two-year institutions. Part-timers are with us to stay and the time is drawing near when we shall have to accommodate to this fact and shape our institutions accordingly.

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DE-PROFESSIONALIZING A PART-TIME TEACHING FACULTY: HOW MANY, FEELING SMALL, SEEMING FEW, GETTING LESS, DREAM OF MORE*

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Part-time teaching faculty in colleges and universities face many problems unknown to (or ignored by) their full-time counterparts. The low status accorded part-time teachers leads to indignities and insults to their professionalism, which they must suffer to keep the jobs that so many others want. University administrators can juggle the number of jobs and the number of people hired to do the jobs so that part-time faculty cost them much less than full-time faculty, especially in lower benefits costs. With little recourse, many part-time teachers become bitter and disillusioned. Their situation is but one symptom of the general malaise of de-professionalization that is infecting university teaching.

Endeavoring to describe the conditions part-time faculty face in universities today is made no easier for me by my recent failure in yet another effort to obtain full-time teaching employment. Such failures only deepen my despair about future prospects, especially when the position I was denied, despite long experience and appropriate qualifications, was at one of the isolated and provincial satellites of the same university that has seen fit for nearly a decade to have me teach the same courses and do the other work the satellite

now associates with a new position. That committed, qualified, and experienced candidates find positions at these remote Dismal Outlets of State U so elusive is characteristic of the problems posed for part-time university faculty in the bear market of the seventies.

However, the diminishing opportunity for full employment is but one of the grim problems part-time faculty face. As I seek, on the following pages, to portray concretely the conditions these teachers experience, it will become apparent that the broadest implications of their situation signal a de-professionalizing of university teaching itself. Part-time faculty daily face a humiliating absence of status, of normal

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amenities, of regard as professionals as well as of even minimally equitable compensation. Our society, already grown more stingy in funding higher education, is thus prodigally destroying yet more of the human capital it has earlier so expensively purchased, while creating a new class of academic proletariat.

My sense of wasted and yet exploited human potential derives partly from my own experience over the past decade as a member of the "Associate Faculty" (i.e., part-time faculty) at one of the largest statewide university systems in the Midwest. But it is a sense reflected also in the experience of my 70-odd part-time departmental colleagues, of the hundreds of Associate Faculty in other departments here, of the few thousand teaching part-time in the system as a whole, and of the huge number of people teaching part-time throughout the nation. Indeed, I am convinced that part-time faculty, sharing similar characteristics as a class apart, disenfranchised, suffer similar indignities and feel similar despair, outrage, and discouragement all across our country.

How Many Are Made to Feel Small

Part-time faculty are a class apart from all other academic appointees. With gestures both overt and covert, both deliberate and unwitting, the university denies them the possibility of strong institutional bonding. Thereby, it begins to undermine their professional and personal dignity.

Perhaps no gesture more clearly indicates the tenuous character of the relationship the university wishes to maintain with its part-time faculty than its form contract. In the nine years of my tenure as "Associate Faculty," I have accumulated more than 25 of these documents, for they are issued for each semester and each summer term, usually in the last two weeks or so before first class sessions. In these contracts, I am "approved as an associate faculty member to teach" specific courses at a fixed "stipend." The term "stipend," though normally a lofty synonym for "salary," here distinguishes me from "salaried appointees" as well as stresses my limited relationship with the university. In fact, the stipend is the same for all who teach the course. After so

many years and so many contracts, the opening paragraph seems to describe my experience less than my feeling of anxiety and the university's wish:

Associate faculty appointments are on a temporary basis in accord with University policy and are subject to cancellation if enrollment is inadequate. Also, if teaching schedules need to be reassigned because of low enrollments, priority will be given to resident [i.e. full-time] faculty.

For many colleagues, both in my department and in others, distressing cancellations and changes of both course and schedule are not infrequent. In my own department, courses have been cancelled because of insufficient enrollment as late as a week after the semester began, which may account for the fact that our department's associate faculty contracts are never delivered until the second or third week of the term. Thus, part-time faculty not only must hang patiently but may find themselves turned quickly away in the shifting winds of enrollment, despite verbal assurance of a contract.

Another paragraph was recently added to this contract:

The appointment to a part-time teaching position . . . is understood to be the only appointment to be approved by [sic] you within [deleted] University. If you foresee the possibility of an appointment on another campus of the university, you should discuss this matter with [deleted] before making any commitment. If any unapproved dual appointment occurs, termination from one appointment may be required.

This paragraph, which seems merely to reinforce the temporary and tenuous relationship the university desires, actually enforces the ceiling the university places on the number of credit hours a part-time faculty member may teach anywhere in the system or, in this case, at its sister university.

For some, the period of anxious waiting at the beginning of semesters diminishes in intensity as re-appointment becomes as regular as tenure, but all feel the outrage of enforced uncertainty, the apprehension that preparatory work for a course may be wasted, and the insulting indifference of never being contracted for a full academic year but always being asked to stay near the telephone the night before

classes begin to learn if there will be work or not.

These contracts fail to mention a host of more covert gestures whereby the university denies the normal expectations of professionals and undermines their personal dignity. Few of my colleagues are more immediately conscious of these gestures than those with extensive earlier experience as professionals. Younger associate faculty and those with limited professional experience may initially seem less aware of the absence of many normal professional amenities. But not forever. One experienced colleague has aptly described the conditions of his part-time teaching as presenting him with almost daily insults to his sense of professional status. These conditions continually call upon him to squander his professional time and prevent him from putting it to its best uses in scholarly preparation, teaching, counseling students and other appropriate professional endeavors. The university seems to assume that teaching labor purchased at so ignominiously cheap rates as I shall discuss below cannot be of professional quality. I even heard a full-time professor publicly express the opinion that part-time faculty must be incompetent, otherwise wouldn't they be teaching full-time? And this despite the fact that many part-time teachers have longer years of service than tenured full-time faculty. To get a more concrete sense of those "daily insults," we need only consider the amenities the university does not provide, denies, or severely limits. At our new campus, where conditions are indeed improved, approximately 200 part-time faculty members from all departments (only part of the total) have "offices" in one large room, divided into some twenty six-by-six foot cubicles, each made smaller by the presence of two four-drawer file cabinets, a flat-top table with a single drawer and two chairs. University space is always costly and in short supply, but six to ten part-time faculty assigned to if not literally crowded into each six-foot-square cubicle would not represent a reasonable cost-benefit to a university that truly valued its teaching staff.

Should concern with space seem unduly petty for professional people, consider the following:

- Only the single assigned file drawer, where still available, can be considered the teacher's private office space. Only it is lockable. Into it must go all teaching materials, books, student papers, even outer clothing during winter months.
- Only one telephone is provided for the use of all part-time teachers assigned to this room. Until recently, no staff receptionist was provided to answer it regularly, so it was usually either in use or incessantly ringing. With so many people served by one phone, inordinate amounts of professional time are spent answering other people's calls. This situation has recently been remedied by the installation of a public address system whereby individuals are loudly paged when the receptionist in the office is answering.
- Only a single half-time secretary is available to part-time faculty. Departmental secretaries are unavailable. Obviously, most of my colleagues type their own copy, make and run their own spirit or mimeograph stencils, transport their own materials to another office to use collators and staplers, and generally do all of their own secretarial work.
- Although full-time faculty and their departments keep the copy machines in constant use, associate faculty use is severely restricted, some colleagues being obliged to pay for copies out of their own pockets. Similarly, a ditto machine provided for part-time faculty was, in fact, the gift of a more affluent part-time colleague.
- Office supplies are simply not provided, although one may personally fetch rubber bands, paper clips, file folders, index cards, yellow pads and note pads from one's departmental hoard, sometimes being called to give an accounting to the departmental secretary. Most bring their own. At any rate, fetching supplies means traversing the length of the building and ascending to the floor of offices for full-time faculty and departments. The gulf between our cubicles and their offices sometimes seems too great and too symbolic a distance to cross to ask for a few paper clips.

Part-time faculty experience these and other similar aspects of their working conditions as expressing the university's disdain and disregard for their professional roles and their personal dignity. That the impact of these covert gestures is deliberate, I usually doubt; that, cumulatively, they add to the sense of low status and less regard with which part-time fac-

ulty feel they are treated, seems indisputable; that they obscure the value of a professional's time and undermine professional development and self-esteem is clear from the sense of "daily insult" the older, experienced professional researcher feels. It was also clearly expressed in a petition signed by most part-time teachers, and even some full-time faculty, when the secretary, whose services were only available half-time, was made even more inaccessible to us by being reassigned to the Dean's office—there to answer the phone and be available for other more pressing duties.

Paper clips, rubber bands and telephones are merely token indications to part-time faculty that they are not thought of as "professionals." There are other, less tangible but more significant amenities of employment denied part-time teachers:

- No promotions to full-time positions. The rule seems to be "Once a part-time teacher, never a full-time one," despite the opinion of full-time faculty supervisors such as mine that no more qualified staff for our program could be found than those presently teaching in it.
- No invitation to other positions within the university to reward part-time faculty dedication and hard work.
- Severe restrictions upon the academic freedom of professionals to develop course materials, choose appropriate texts, and direct their classroom teaching as they choose.
- Normally no participation in the professional self-governance of their professional work.
- Discouragement of any desire to develop and offer new courses for which one is specially qualified. The most outrageous recent incident I know of happened to a colleague who is nationally known as expert in a special area of sociology. A course he proposed was refused, only to be listed as an offering for the coming semester by the full-time colleague who had discouraged his proposal.
- No recognition by the university of outstanding achievement, either by award or commendation.
- No leave of any sort, except informally without pay, of course. The part-time faculty contract explicitly states the teacher's personal responsibility to provide "an acceptable substitute should any emergency prevent . . . meeting a class"; and this includes sick leave, military duty, and jury duty.
- No provision for professional travel, for attending conferences.
- No financial aids for professional development and research. Access to the grant-proposing, assisting and evaluating apparatus of the university is specifically barred except via the co-support of a full-time faculty member, few of whom will sign much less encourage grant proposals by part-time faculty.

By these gestures part-time faculty are refused institutional affiliation and find their professionalism ignored, their professional affiliation or development discouraged, and their human dignity so questioned that feelings of despair, self-doubt, outrage, distress, paranoia and hopelessness are, I believe, universal. Some part-time faculty express these feelings, but many repress them and suffer their pain and humiliation in silent, alienated indifference. My theme is that the alienation of these professionals as persons is in reality a sign of an increasingly widespread de-professionalization of university teaching itself.

How Many Are Counted as Few

These gestures, some deliberate and some unwitting, are but symptomatic of conditions created by another, larger gesture that is both covert and unquestionably deliberate. The first may help explain how the many part-time teachers at my university are made to feel small; the other not only accounts for their negative feelings on a far broader scale, it also defines the entire relationship the university establishes with them and determines the compensations and other benefits they will receive. It further shapes those attitudes toward part-time faculty that deny them institutional bonding in large and small ways and that accord them little status or regard as professional workers. Perhaps the most remarkable and desirable effect of this gesture—for the university—is to make the many people relegated to part-time teaching appointments seem so few.

Nowhere in my contract with the university is the ratio stated between the amount of work I am appointed to do and the amount expected of a full-time member of the faculty. Yet the Full-Time Equivalency method for apportioning the time

and duties of employees, for providing a uniform standard to determine the size of a work force, and for creating equitable scales of compensations and benefits is almost universally used throughout large institutions of every sort, including this university. Why, therefore, does this university not openly declare the Full-Time Equivalent (FTE) of the work it assigns part-time faculty? The answer to this question may at first seem puzzling despite its brevity. The answer is this: it does not matter what proportion part-time faculty work, for it will always measure less than 50% FTE. There must be magic here—and there is.

At my university, and at many others, 50% FTE is the mystical Rainbow Bridgehead to a Valhalla for professionals and “real” employees of the university. We in the nether regions look toward that domain and see a golden land replete with ivory towers whose residents sit within in deep contemplation. State legislators, trustees, and administrators at every level, charged with maintaining this domain, well know the expense of ivory towers and the upkeep of their occupants in this present age. And so they have discovered a plan to preserve Valhalla’s glory and its high standard of excellence. If they can keep a large work force busy below 50% FTE, give them more and more of the work to do, and keep them ignorant of exactly how much of the work they are doing (for that knowledge breeds dangerous and expensive aspirations), then the privileged life in Valhalla’s towers may continue despite inflation’s threatening Twilight and declining enrollment’s erosion of the glittering Bridge.

Perhaps 50% FTE began to acquire its magical force as long as 40 years ago when universities first began to enroll in the then new Old-Age, Survivors, and Disability Insurance program (OASI), more commonly known now as Social Security. In the special agreements institutions entered into with the Federal government, certain classes of employees could be excluded from the program—specifically those classes labeled “temporary” and working less than half-time (now known as 50% FTE). From the model of that exclusion (originally intended to protect a recovering economy) seems to have arisen

the pattern of all subsequent exclusion of part-time employees from the customary benefits of employment. As long as such employees could be designated less than 50% FTE, and in some cases kept to very short terms of employment such as a semester, they could be excluded from the benefits coverage of whatever new program the university might be impelled to enter. And as long as part-time faculty could be excluded, they could be cheaply purchased and dearly used. They are the industrious Nibelung dwarves who, if not always mining the richest ore, at least can be kept in the dark, consuming little of Valhalla’s wealth.

Wherever the university might incur a direct cost, part-time faculty are excluded:

- No Social Security is deducted or paid;
- No Group Life Insurance is provided or accessible;
- No Group Medical Insurance is accessible;
- No Unemployment Insurance is provided;
- Neither of the two pension plans, TIAA/CREF or PERF, is accessible;
- No certain coverage under Workmen’s Compensation and Occupational Disease Insurance is declared.

Of all these exclusions, the denial of Unemployment Insurance to a class created *by the university* and kept by it perpetually in more danger of unemployment than any other is perhaps more offensive even than the exclusion from Social Security.

But those in power have yet another card to play. Part-time positions are usually publically expressed only as 100% FTE positions being paid on a part-time budget line. Thus, the administration can arbitrarily reduce the FTE value of a credit hour and employ still more persons part-time (at less than 50% FTE) without increasing the number of 100% FTE positions being paid part-time. Complaining accrediting teams can be assuaged without ever addressing the reality that 200 such positions mean at least 400 teachers and probably closer to 600. By reducing the value of a taught credit-hour from 25% FTE to 20% FTE, as my university recently did, the 200 *positions* may not change in the budget, but a much greater number of people can be employed to fill

them—cheap people, Best Buys, people already masters of the subsistence life-style, people who eat little, dress poorly, take little pleasure in luxuries like job security, homes, cars, can think little of themselves for accepting such contracts anyway, people who will merely “teach.” And that is how the many become few.

How These Many, Becoming Few, Get Less

Being accorded respect as professionals might buoy their spirits, but respect and amenities alone cannot compensate part-time faculty for their wages—which bear no relation to those paid full-time faculty. Just how inequitable their compensation actually is may not seem immediately clear. At my university, the mean wage for each budgeted part-time position, rated at 100% FTE, is (or would be if there were any such creatures as 100% FTE part-time appointees) about \$6300 for the academic year. The mean wage for each budgeted full-time position, rated at 100% FTE, is (and this figure is close to actual) about \$17,500 for the academic year. Before concluding that a triple pay for full-time faculty is pretty much balanced by the triple responsibilities (teaching, research, and service) they alone are supposed to bear, the reader must recall that the 50% FTE barrier means that no part-time faculty member can earn the mean wage for the position. In fact, only by adding on summer wages can any part-time faculty member raise the annual wage above about \$3500. And all this time, part-time faculty are teaching 40% or more of all credit hours each semester at my university. Very many individuals teach two and three courses each semester as well as perform other services that include but are not limited to sharing in committee work, counseling students, attending committee and faculty meetings, keeping abreast of scholarship, and engaging in work-related research and writing—for an FTE rating recently and arbitrarily reduced from 50% to 40%. None of these individuals will receive more than about \$3500 for the academic year. Just how much of a Best Buy this university has is shown in the aggregate costs of faculty instruction: 40% of all

credit-hours, taught by part-time faculty, cost only \$1.2 million, while \$7.25 million is spent on the 60% of credit hours taught by full-time faculty. Employers who would exercise every skill to deny status to an entire class of employees upon whose services they are unavoidably and increasingly dependent, and who would hide behind the protection of laws designed to prevent just such practices, and who would then further deny the same class of employees even the figment of a fair and equitable wage, are indistinguishable from thieves and criminals.

Anticipating one possible objection, let me add that even if part-time faculty responsibilities were restricted to teaching alone, which they are not, and if the general level of credentials (though decidedly not experience) more closely approximates that of junior faculty earning \$12,500–\$14,500 per academic year, \$3500 maximum earnings for a 50% FTE position without access on a prorated basis to any other benefits is still a grievous injustice. A 100% FTE load for a full-time appointee is rated at twelve credit-hours, of which nine to eleven are normal teaching loads, additional hours being designated to cover other responsibilities. Part-time faculty who teach six or seven credit-hours each semester in addition to other responsibilities cannot be said to bear only one-third the responsibility of full-time appointees. Nor are their credentials paltry. Among my 70 departmental colleagues, all possess advanced degrees and often uniquely qualifying experience; an increasingly significant number possess doctorates; many are ABD. In another department, *all* associate faculty now possess doctorates but can still only earn \$3500 per academic year.

Although my university ignores the fact, it is well aware that for most of its part-time faculty, the earnings received from this professional work are a major, significant, or sole source of income. Instead, the university chooses to view all as sharing the condition of some part-time faculty: those principally teaching evening courses, for whom teaching is a secondary source of income. Hence, the prevailing fatigue into which the despair, alienation, anger, and distress of part-time faculty finally seems to merge is brought

on not merely by the demeaning regard the university has for their professional aspirations. It arises as much from the awareness they have that when the last student is counseled, when they have finished a stencil they will run off in the morning, when they have completed synthesizing three recent articles to supplement a lecture on the long chapter also assigned, their compensation for these professional labors does not permit them to rest. They must now hurry to the bar where they tend tables until 2:00 A.M.; or to Mrs. Lester's house to mind her kids for the evening; or to work as a hairdresser, retail clerk or secretary to supplement their income. One PhD in my department has been a para-legal for several years and is now in law school. Only a fortunate few are able to deplete their personal energies in more attractive supplemental work writing grants, community publications, or performing free-lance editorial, consulting, or other professional work. I speak not, of course, of those part-time faculty who do have another principal source of livelihood—for them, part-time teaching may ironically be the means of fulfilling part of their principal professional obligations elsewhere.

When the Many, Getting Less, Dream of More

It cannot be said that part-time faculty in my department, at least, lack imagination to remedy their condition. One recent effort has a comic flavor fitting for a conclusion. This Pirandellian interlude might be entitled "Sixty Teachers in Search of a Rank."

This group of part-time faculty, desperate for secure employment, have allowed themselves to be ensnared in a cost-free incentive system that has extracted from them a substantial, but entirely uncompensated, gain in productivity. Devised by the departmental director of this group of associate faculty, and originally intended by him merely to reduce the chaos of twice yearly hiring of 70-odd part-time teachers (a task created by the administration's single semester contract limit), to

improve "traffic" as he says, and to make a tacit commitment to hire seem more explicit and more certain, this system assigns "points" to each part-time teacher for education, experience (weighted to favor this program), service, and publication. The weighting of the "point system" was quickly perceived as ensuring that the relative position of each of these part-time teachers remained nearly the same as before; therefore, no real gain in employment security had been achieved for those with fewer or fewest "points." So it was soon decided that "ceilings" should be placed on both education and experience "points." Those attaining such ceilings, and each semester their tribe has, of course, increased, find that now their only means of further maintaining or improving their positions relative to each other lies in garnering service "points." The result has been a truly extraordinary increase in productivity of every sort for which "points" are awarded without the cost to the university of an additional budgeted dollar. Such is the desperate need of these part-time teachers for even the fraction they receive of an equitable wage, that to guarantee they get that, they have for several years now rendered my department hundreds of hours of additional and non-contracted labor in exchange for an unenforceable promise, symbolized by immaterial tokens that are neither legal nor negotiable tender for anything. Because this "point system" guarantees but an unenforceable promise, contingent, as all part-time appointments are, upon enrollments (including those of full-time faculty members who, should their courses fail, may need my job); and because enrollment has, during the same period, continued to increase generally, assuring not only continued regular employment but yearly increases in part-time staff—residents in Valhalla's ivory towers always smile when mention is made of the "point system." An Associate, returning to the dusky region beneath 50% FTE, claims to have overheard the chanting refrain, "Let 'em eat points."

COMMENTS

Regardless of one's position in this society, it is abundantly clear that there are serious problems in the economy and that full employment has continued to be elusive. One manifestation of this is the growth of part-time employment, occasionally identified as under-employment. Those who work in universities have become especially aware of this phenomenon because of the dramatic trends which have shaped the academy. While the 1960s experienced the opening of one new college or university in the United States every week, every year throughout the decade, the 1970s have witnessed the shrinking of the (traditional) college-aged population and the non-growth or contraction of enrollments. Furthermore, as most faculty joined the ranks since 1960 there is a very modest attrition by death and retirement and hence few new positions in university facilities. We might then welcome attention to this reality, especially if we are provided with interpretations and possible policy implications.

My first query about the Tuckman, Caldwell and Vogler and Van Arsdale essays is whether to fault the authors for what they do *not* do. That may be done quite legitimately; the lack of analyses here is significant. In fact, the issue of how Lipset and Ladd allegedly ideologically slanted their political questions in their survey of university professors (Lang, 1978) is similar to the failure in these papers to treat the structural, economic and behavioral causes and dynamics of the problem of part-time employment. The fact that the first authors are economists and the second is in the field of English is not relevant (see, for example, another approach in Ohmann, 1976). What we have in these two essays are interesting and important descriptions of the phenomenon, but little analysis of the economic and structural reasons for it, from neither the more detached parties nor the aggrieved and victimized author.

Tuckman and colleagues point out that part-time employment does not help in the pursuit of a permanent and full-time academic career. Yet nowhere do they detail the factors in the labor market which have compelled most part-time faculty to make this accommodation. We do not get a sense of the general awareness around universities that in contrast to the 1960s when there were two jobs for every new PhD, now there are two doctoral applicants for every position. Furthermore, there is no mention of the cost-savings approach in higher educational institutions. The authors refer indirectly to the policies of institutions that hire part-timers, but there is no mention of the pattern towards policies that are cost-savings, policies

that reflect a "buyer's market," or policies that are openly sexist in their exploitation. In short, it is mostly what we do *not* get that is disappointing. To be sure there are some interesting facts, but here we have a national sample of 3,763 persons in 128 institutions, from which we should derive more than descriptive profiles. What would we have if the authors examined their data in relationship to national labor market trends, evidence of institutional policies which are exploitative, and future policy trends? It would be a more interesting report and a better piece of social research.

I certainly can appreciate the passion and feelings in Van Arsdale's paper. Most of us can identify some of the outrages he describes in our own settings. Still, that is the problem, for he too only describes the situation out of his experience and does very little sociological analysis. Some small questions: If pay for part-time faculty is comparable to full-timers, does that remove the demeaning aspects of their role? Is there a special situation in some departments, such as English with its typically required courses and enormous enrollments and the unusually large proportion of part-time faculty employed? Does that relate to the unusual rate of sexist exploitation of part-time female "readers" and "instructors" usually found in English departments?

Van Arsdale comes close but never really details the labor market trends, unemployment and job insecurity, the pressures to economize and the relationship between national economic and political trends and policies in higher education. He does make some especially valuable points and a strong condemnation of the exploitative phenomenon that most academics know but would rather not have to admit and deal with. Given the leveling off and drop in the number of 18-year-olds and growth of interest in adult education, "non-traditional learners" and life-long learning, we must recognize the resulting cost squeeze. These programs are typically self-financing and have modest fees; thus the college or university is especially anxious to use "cheap labor," that is, part-time faculty.

A few points are essential for understanding the phenomenon of part-time employment. First, there is the reality of the economy itself, which continues to function with massive unemployment. While today's headline may suggest a modest reduction, in fact the problem is no less severe. The evidence is clear that even with new U.S. Department of Labor categories such as "discouraged workers," there is substantial underreporting of the true rate of unemployment (Leggett and Gioglio,

1977). Furthermore, the events of recent months portend of catastrophic fall-out from the so-called "tax-payer's revolt." A colleague has stated that the Proposition 13 vote in California is the first time an industrial society has voted itself a depression. In any event, the impact of the current electoral mood suggests further reductions in financing public services, which means that education and related public employment will be even further affected and the labor force more skewed in terms of supply and demand.

Although Tuckman and colleagues and Van Arsdale do not suggest it, the fact is that a number of manpower specialists have advocated part-time employment as a means of coping with high levels of unemployment. This has gained especial attention in the European Community or Common Market Countries (Social and Labour Bulletin, 1978), but has been talked about in Washington as well (Levitman and Belous, 1977). Of course, what is suggested is a policy of "sharing the work"; namely, attempting to provide employment for as many people as possible even if that requires reduction in work-time and a move to part-time employment. The theme sounds equitable, but it is difficult to imagine fully employed tenured professors voluntarily dropping their FTEs (and salaries) to hire some presently unemployed academics. The dilemma is that the economic and political structure is deficient and asks citizens to sacrifice to spread limited rewards among the population, rather than restructuring the balance between needs, services and employment opportunities. Ask an inner city school teacher, social worker or nurse if there are too many professionals in that field! View the deteriorating quality of the environment and ask if there are too many people working in environmental control and forestry!

The problem is that the political economy is shaped so that we are "oversupplied" not only with unskilled labor, but also with an educated populace. In the 1960s when poverty was "discovered," films could be made with titles like "Superfluous People," depicting the automated poor, masses pushed off the land, and the unskilled and semi-skilled unemployed. In the 1970s the labor force is structured differently; educational attainment has increased and the permanent problem of unemployment in the United States has affected college graduates and professionals. The debate should not be over whether college was "oversold," but over how to restructure the economy to better serve human needs. In the meantime, the reality is that academics are in over-supply for existing employment and that part-time employment will be a feature of the economy for important structural reasons. I just wish

that the sexism and economic exploitation would be sociologically examined instead of merely described.

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During the month of August this year I talked with nine persons who are recent part-time teachers in sociology in the Chicago area. Their experiences suggest to me that the existence of the part-time job system and particularly its expansion at the expense of full-time jobs poses serious threats to the profession.

Most of the part-timers I talked with are not hired to fill in for on-leave full-time faculty members. Rather they are temporarily hired to fill more or less permanent part-time job slots. Some of these jobs are located within special divisions called such things as extension program, evening school or weekend college. Others are located in the regular programs of colleges, which appear to consist of a few full-time faculty (who may either double as administrators or be former administrators) and a large administrative staff to recruit students and hire the part-time faculty to teach them.

Contracts, when they exist, are contingent upon the attainment of some minimum class size. They bind the individual part-timer to teach the course but do not bind the institution to offer it or to offer any future employment. These part-timers report receiving between \$750 and \$1200 per course for courses taught in

1977-78 or contracted for in 1978-79. Because most part-timers do their own grading and often their own office work, estimates of hourly earnings range around \$10. At this wage, a 12-months salary for the equivalent of a full-time load (4 courses per semester and 2 in the summer) would range from \$7500 to \$12,000.

The job market for part-time positions appears to be not only local but also largely informal. By my own observation, vacancies are rarely advertised. Institutions appear to rely on the initiative of applicants and on contacts with the various graduate programs in the city. When an institution has its own graduate program, part-time teaching slots may be used to support its own graduate students. To the outsider, the goal of the job search appears to be locating the most conveniently and cheaply obtainable minimally qualified warm body.

While my informal survey does not permit me to demonstrate the operation of discriminatory practices in filling part-time positions, it does suggest that the formal procedures that guard against discrimination in hiring regular full-time faculty are typically not operative in hiring part-time faculty. Even the normal procedures for participation by full-time faculty in hiring are often not invoked in filling part-time vacancies. In short, there are few institutionalized limits on the administrator's discretion in awarding such positions. It is obvious that this system offers little protection for the academic freedom of part-time faculty.

The efforts expended to recruit part-time faculty stand in sharp contrast to the efforts to recruit the students they will teach. Evidently the latter are in relatively shorter supply than the former. The programs are hustled on placards displayed on public transportation, in newspaper advertisements, by direct mail and on radio spots. They promise "quality education," "encounters of the best kind," and affordable tuition ("If you can afford night school, you can afford [Whatever U.]").

The hard sell and the contingent contracts requiring minimum numbers of students suggest what is at stake. Most of the part-timers I talked with teach in programs that are run as money-making enterprises by the colleges involved. Moreover, the cost accounting is applied at the level of the individual course, which is not offered if it does not draw enough students to show a profit. This means that courses are not offered based on program balance or for other academic reasons. Rather they are offered because they consistently draw enough students to make money. If the individual instructor wants a course to "succeed," then he or she is constrained to make it popular. Being personally skeptical about

course "popularity" as a criterion for educational quality, I believe that such a system inevitably undermines academic standards.

Other aspects of the structural situation in which most of these part-timers work are detrimental to educational quality. Not knowing in advance whether a course will be held, the part-timer cannot afford to invest much time in classroom preparation. The course format often consists of a three-hour, once-a-week "one night stand." Nearly all the part-timers I talked with feel that this format is a serious constraint on both the material covered in a course and on what they can expect from students. The fact that both the students and the faculty member are typically involved in full-time jobs or studies elsewhere limits the time available for out of class preparation and reading.

Failure to attend to specialty qualifications represents another assault on program quality. Some programs attend to specialty qualifications in hiring and others do not. Part-time faculty who wish to remain regularly employed have to be willing to teach a variety of courses. Several part-timers report teaching in areas where their own backgrounds are minimal.

If university professors had an effective union, we would require members to forgo part-time nonunion employment, which drives down the wage and removes work from the work pool. Lacking such regulation, each of us should carefully consider participation in a system that potentially undermines educational quality, academic freedom, equal opportunity employment and our collective and personal economic positions. Senior faculty and full-time faculty should resist administration efforts to exploit the part-time system. Those who participate in accrediting reviews should look carefully for this kind of abuse. Those who teach part-time should consider the effects of taking a low wage and less than professional employment conditions. While the faculty victims of the part-time system are not responsible for the conditions giving rise to it, its abuses would not be possible if there were not so many of us willing to work for so little.

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The relationship between private troubles and public issues links together a compelling account of a career on the fringes of academe and a composite portrait of academic part-timers. The information provided and the con-

clusions drawn by Van Arsdale and by Tuckman, Caldwell and Vogler allow us to address an issue of theoretical and substantive interest to sociologists—the generation of internal labor markets within higher education. Recent scholarship (Edwards et al., 1975) suggests that background variables combine with market forces to create strong boundaries between two categories of workers, the “marginal” and the “guaranteed.” The two differ not only in current status and benefits, but also in long-term job security and prospects for advancement. The part-timer fills a special role in academia by teaching unstaffed courses, thereby helping departments and colleges to maintain their enrollment figures at a relatively low cost. At the same time, the institution has only few obligations to the part-time, marginal instructor. Generally, distinctions between marginal and guaranteed workers are self-reinforcing and grow sharper in periods of scarcity.

To elaborate this point, the academic part-timer might be defined both more narrowly and more broadly. I would include those who would prefer a full-time position and whose qualifications are similar to professors in tenure-track positions. By his own account, Van Arsdale would fall into this category, as would many others whose subsistence depends on part-time teaching. I also suspect that the term “Hopeful Full-Timers” underrepresents marginal academics. Teachers who fill one-year or one-semester nonrenewable positions, that is, the academic gypsies who migrate each year to temporary jobs, should also be included. By the same token, some part-timers are not marginal. It makes sense to exclude tenured professors whose family income allows them to cut their teaching and other academic obligations, professors who negotiate long-term job-sharing arrangements, and graduate students who teach part-time for the experience or while completing their dissertations.

We cannot fully understand the academic market's workings from the data reported here, since they do not include an assessment of those variables that may predict full-time or part-time employment: race, sex, parents' education and occupation, and the individual's training. Similarly, we are not told how teachers are initially recruited into part-time teaching or the extent to which current part-timers have made this type of teaching a “career.”

The perquisites of guaranteed academic positions are obvious to both full-timers and part-timers. They include private offices and phones, at least some secretarial assistance, partial choice of courses, some control over time and schedule, the option of individual re-

search or collaboration with the colleague(s) of choice, and the franchise to influence a department's direction. For many of us scholarship is both a vocation and a career, and a guaranteed position provides the amenities, as well as the time and security, for continued academic growth and development and an increased opportunity for mobility.

Van Arsdale's list of the indignities and inequities faced by part-timers is long and exhaustive, but his emphasis is on day-to-day problems that may be faced over and over again. While he is concerned primarily with remuneration, convenience and status, the differences between marginal and guaranteed academics may more closely approximate class or even caste distinctions. The perennial marginal academic has limited opportunities to enjoy the type of professional growth that enhances his or her chances for landing a guaranteed position. Research activities are indeed more difficult without the support, facilities and legitimacy a university can furnish, and complications increase if the part-timer moves from school to school and city to city. Collegial assistance may be perfunctory at best.

Teaching is even more problematic. Basic or introductory courses can be pedagogically exciting, yet still not contribute to intellectual needs. Marginal academics teach the courses they are offered, not necessarily those they wish to teach. Thus teaching does not provide an entree into a new area of intellectual interest as it may for guaranteed academics. Scheduling and even the choice of texts is often at the behest of the institution, not the instructor. Moreover, there is frequently little time between the date of hiring and the first day of class, which discourages careful course planning. In sum, part-time teaching provides few developmental spin-offs and at best only qualifies the individual for further part-time work in the future. Even those who prefer teaching over research suffer from the disadvantages of marginal work.

The projected scarcity of full-time regular positions in the academic marketplace of the eighties suggests these problems will continue. As the proportion of part-timers increases, pressures of supply and demand will weigh heavily on more potential academics than ever. Part-timers may come to resemble the artisans in cottage industry, taking work not when they want it but only when it is offered. Because college teaching is still a desirable and rewarding occupation, their proportionate number may increase. But in times of budgetary restraint, institutions may have little incentive to improve stipends and benefits. The cynicism expressed by Van Arsdale may come to be shared more widely in the period that lies ahead, particularly if a large percentage of new

PhDs must reluctantly choose nonacademic positions and as a smaller percentage receive tenure. The hopeful recommendations of Tuckman, Caldwell and Vogler would, if adopted, substantially ease the day-to-day situation of part-time academics. They would, however, do little to retard the process of marginalization or to bridge the real gaps between marginal and guaranteed teachers.

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From 1970 to 1977, I was a part-time instructor at seven junior colleges, colleges and universities, teaching as many as five classes a term at three different institutions. The fundamental concepts developed by social critics (see the *Radical Teacher*, July, 1977)—exploitation, alienation, oppression, proletarianization, and labor solidarity—proved most useful to me for analyzing work within the walls of academia.

Part-timers have always been an integral portion of the workforce in higher education, as moonlighting professionals or businessmen, as graduate student assistants, and as visiting distinguished scholars. The problem is the proliferation of the Semi-Retired, Students, Hopeful Full-Timers, Homeworkers, Part-Mooners and Part-Unknowners in Tuckman, Caldwell and Vogler's taxonomy. When college enrollments soared, "temporary," "associate," and "adjunct" positions were justified by top administrators as a supplementary shift that would stabilize the workloads of regular faculty. Now, after several years of declining admissions, retrenchment, reorganization, budget crises, cutbacks, hiring freezes, inflammatory tuition hikes, and misdirected taxpayers' revolts, part-time ranks continue to grow as substitutes to fill vacancies caused by attrition: deaths, retirements, resignations, illnesses, sabbaticals, dismissals. Administrators seek to hire part-time rather than full-time labor during periods of expansion and contraction, within the limits set by accreditation guidelines.

The new wave of part-timers experience situations analogous to the conditions endured

and then opposed by other traditionally exploited workers in factories, fields and offices. Part-timers liken themselves to an "academic proletariat" of "intellectual pieceworkers" hired to complete a scheduled course; "intellectual clockpunchers" paid by the contact (credit, semester) hour; "itinerant teachers" who migrate from employer to employer seeking seasonal labor; and "Rent-A-Prof" or "Professors Temporary" on stand-by status awaiting a phone call. Part-timers grudgingly accept harsh terms—low wages (stipends), no fringe benefits, no cost-of-living adjustments, no merit increases, no increments for accumulated experience—because they are desperate and disillusioned victims of double digit inflation and an "overproduction of PhDs" by the very same universities that use them as cheap, flexible and dependable labor.

Administrators erect structural barriers that alienate part-timers from their work, students and colleagues by hiring persons solely to teach one or two specific classes. Part-timers are denied research grants, free computer time, copying allotments and travel reimbursements. They become estranged from their professional field, and the fulfillment of their potential is impeded. Administrative policies that refuse part-timers office space, telephones, compensation for counseling and supervising independent studies, and advance knowledge of next term's assignment(s) cut these teachers off from their students. Administrative vetoes of their representation in collective bargaining units, inclusion under regular faculty contracts, and participation in departmental and institutional governance committees drive wedges between part-timers and their full-time colleagues. Part-timers (and department chairpersons) confronting these administrative hurdles face a dilemma: either teach and run, thereby minimizing exploitation but maximizing alienation; or stay and integrate, furthering exploitation but reducing alienation. The isolation of part-timers from the academic community is intensified if they must meet their classes in remote buildings, rented off-campus sites, satellite extensions and/or teach very early in the morning, at night, on weekends, during summers or intersessions.

With the start of every semester, part-timers are reminded of their frustrating powerlessness. They anxiously wait for last minute opportunities, scheduling conflicts to be resolved, funding to be confirmed, their classes to fill, and for the courses of regular faculty to get sufficient enrollments (cancellations trigger a process of "bumping" that leads to dumping). Part-timers have very little control over their own laborpower.

Job insecurity is the crux of part-time oppression. The "self-terminating memorandum

of appointment" that guarantees no rights and no due process or grievance procedures generates second class citizens who exercise academic freedom only at their own peril. Those who organize in behalf of their rights are vulnerable to political repression every term, and can be purged through administrative subterfuges like insufficient funds, unapproved budgets, or even affirmative action searches. Women, racial minorities, younger and older persons, and political radicals are disproportionately concentrated within this lowest stratum, so their oppression within academia is an extension of larger patterns of discrimination and marginalization.

The proletarianization of part-timers—the stripping away of pay, power, privileges and prestige (Van Arsdale's "de-professionalization")—is the direct result of the invasion of scientific management practices (specification, simplification, standardization, routinization, centralization) in pursuit of cost effectiveness and productivity gains. The most ominous developments to date crop up in off-campus evening programs for special students (e.g., veterans, nurses) run by private, profit-oriented junior colleges. The entire staff of isolated part-timers at scattered rented sites are underpaid by the contact-hour strictly to teach over and over again an introductory or survey course from an imposed textbook with uniform exams.

The degradation of the process of higher education can be reversed if professional organizations, faculty unions and full-timers overcome their divisive ambivalence towards their fellow teachers, reject the administrators' ideology of "blaming the victim" for marginality, and unite in solidarity with part-timers by incorporating them into their collective bargaining units, their departments and their policy-making committees. The goal is not to abolish reduced workloads or eliminate part-timers; it is to end forced underemployment by regularizing half-time (or third-time or two-thirds-time, etc.) positions: expanding responsibilities beyond just teaching, upgrading pay, prorating benefits, and securing rights to due process, promotion, and tenure. I hope the impassioned indictment by Van Arsdale, the dispassionate multi-variate dissection by Tuckman, Caldwell and Vogler, and these comments will galvanize support for very needed changes.

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The paper by Tuckman, Caldwell and Vogler appears to confirm something most of us suspected: the sub-class of academic coolie labor has undergone an ominous transformation. "Part-Mooners" and "Hopeful Full-Timers" now outnumber graduate students—the traditional source of cheap academic labor—and the three categories together constitute over half the part-time academic workforce. Increasingly, these part-timers are people with degrees, not apprentices, people for whom there is no full-time work, who work in degrading conditions, and whose very existence poses a threat to full-time academic employees.

The authors, to their credit, do not blame the victims for this sorry state—unlike the present Commissioner of Labor Statistics who attributes our high national unemployment rate to women who insist on leaving the kitchen to look for work. No, what we have here, the authors tell us, is the market at work, a classic case of supply exceeding demand. This in turn produces falling wages, worsening working conditions, and all the rest of the dismal but entirely predictable consequences of too many workers competing with each other for too little work.

Nonsense. That there are too many people looking for too few full-time jobs is not in doubt. That the lack of full-time jobs is due either to an oversupply of candidates or to a lack of positions is open to a great deal of doubt. It is doubtful because the authors' own figures indicate an increase in the number of positions of all kinds at all levels of post-secondary education. Specifically, they show a displacement of full-timers by part-timers at universities—a retrenchment of *persons*, not *positions*—and a huge growth in two-year institution positions, which are filled, however, almost entirely by part-timers.

What this suggests to me is that even if the number of qualified teachers only kept pace with the number of openings—if, in other words, the authors' suggestions for enforcing guild restrictions managed to bring supply more in line with demand—the transformation of full-time positions into part-time positions would still continue. Why?

Because we are employees and therefore represent a cost, an expense to be reduced when not completely eliminated. This equally predictable consequence of the marketplace applies no less than the "laws" of supply and demand. I should add that it also applies to academics no less than to employees of profit-making firms, whatever the supply of job-seekers, the supply of jobs, the state of the economy, or the level of technology. I will provide an example from an industry which, at first glance, seems light years away from teaching.

The electronic data processing (edp) industry—computing—is to the second half of the 20th century what the auto industry was to the first half and the railroads were to the 19th century. It is the boom industry of our era and it could be fairly said to keep the economy going. Even during the 1970-75 bust, the computer industry suffered only interruptions in its growth rate. It embodies the technological edge which separates our economy from that of most of the rest of the world. The computer industry, in other words, is growing, is capital (technology) intensive, and suffers, furthermore, from chronic labor shortages in almost every area: manufacturing, clerical and administrative, as well as engineering and scientific. In short, the edp industry is in every important respect the opposite of teaching. Yet, the industry is characterized by a rapid process of routinization and de-skilling of its technical workers (Van Arsdale's "de-professionalization"), the sub-contracting of work to outside organizations, and the use of temporary workers who are paid less than other company employees, denied "fringes," and excluded from coverages like unemployment, accident, health, and workmen's compensation insurance. My own research indicates a wide-spread and systematic use of these "temps" by employers who, in effect, can have a full-time workforce for a wage bill considerably less than for full-time workers. In my state the law is generous. It allows the use of the same cheap and fringe-free workers for half a year—even longer under some circumstances. While this provides the much vaunted "flexibility" for managers seeking seasonal help, it also allows employers to maintain a continuing low-paid workforce by replacing one temp with another from a rotating pool.

If, as it happens, there is a full-time opening, the temp pool provides the employer with a handy, low-cost and relatively risk-free way of trying out potential full-time workers, without the bother of fringes, unemployment and workmen's compensation insurance premiums, etc.

Sound familiar? And while the immediate incentive in the edp industry to do this—high labor costs due to shortages—is precisely the opposite of what faces college administrators with their "lack of demand," the underlying thrust is exactly the same: to cut costs. If there are no profits to increase or shareholders to please, there are still expenses to trim and trustees to mollify. In either case, managerial cost-cutting techniques are the same.

Van Arsdale's eloquent account shows us what some of these techniques and their effects can be in the academy: fostering competition to score brownie points with administrators, lowering the FTE values of part-time courses—what in

a different kind of workplace would be called chiseling on the piece-rate—and a perverse sense of hope which makes abuse easier to endure as long as there is a promise of another crumb falling from the academic table.

On the other hand, I can't sympathize very much with Van Arsdale's lament for the loss of "professionalism." We are, or were, privileged workers, accustomed to being hired, fired and occasionally favored one at a time. We flattered ourselves by thinking that our fates were determined exclusively by our individual merits, or at the very least by our individual bargaining abilities. We should be thankful to Tuckman, Caldwell and Vogler, and to Van Arsdale, for pointing out that there are forces out there that make such individual deals less and less likely for even the most meritorious. As long as we pretend that because we are "professionals" we are therefore not also employees, as long as we prefer to deal individually with our employers rather than collectively, as long as we condone the existence of a permanent underclass of badly paid, over-worked, dead-ended, and disenfranchised colleagues, then we will join their ranks before they join ours.

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These two very different papers provide valuable information about part-time employment in academia. The need to understand this phenomenon increases as its incidence increases. The authors tell us the extent to which part-timers are underpaid, overworked, unappreciated, and generally alienated. Each paper takes a different approach to the topic, but the picture is the same—the part-timer works under very poor conditions.

An accurate portrayal of the situation is, of course, valuable in and of itself. Prodding the various disciplines (or academia as a whole) to improve the situation is a possible next step. To reach this next step, further research is needed. In particular, we need a more accurate picture of who these part-timers are, perhaps legitimizing their claim on professional resources. (Ideally, the simple fact of their existence and need should justify their claim. However, the historical fact that graduate students are paid low salaries and work under poor conditions in return for providing valuable teaching services indicates that a legitimizing process is needed. Academia, as all social systems, does not necessarily tend toward "just" solutions.)

Most people probably think of part-timers in

academia as students who have not yet finished their degrees (and so, presumably, in temporary situations) or as persons with full-time jobs who simply enjoy teaching an occasional course. Whether or not this perception was ever accurate, several recent trends make it an invalid assumption about the future. First, many well-qualified professionals may be able to obtain only part-time positions because of the shrinking academic job market (caused by falling student enrollment)—there may simply not be enough full-time positions to go around. In addition to this, there is the increasing number of married women with professional degrees who move with their husbands to places where they cannot find full-time positions. (The simple increase in the number of women obtaining professional degrees makes an increase in professional women following their husbands likely.) Both groups can certainly make more “legitimate” claims on professional resources than students who have not finished their degrees or persons who have finished but are not committed to pursuing this career full-time. Showing that part-timers increasingly are dedicated professionals who are unable to secure full-time employment might increase efforts to improve their situation.

Neither paper deals with this issue. Even in Tuckman, Caldwell and Vogler’s paper, where various categories of part-timers are considered, people with varying motivations are mixed together. For example, the “Hopeful Full-Timers” and “Full-Mooners” may include women who have moved with their husbands to places where they cannot find suitable full-time employment. The latter category may also include persons who cannot find full-time jobs in their professional areas and seek part-time jobs there instead. To give a clear picture of who these part-timers are, such distinctions should be made in future research.

The sex distinction is especially important, since women typically fare more poorly than men in the labor force, and certainly may do so here. Surprisingly, this possibility is not considered in either paper—nor is the possibility that women are comprising an increasing (or very substantial) proportion of the part-time population. Making these distinctions, and considering these possibilities with a more representative sample of the part-time population (a weakness of the Tuckman et al. paper), will certainly further the part-timers’ cause. Such an approach must build upon the considerable groundwork laid by these two papers.

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In the 1960s Herbert Gans coined the term “malemployment” (Gans, 1968) to refer to jobs which offered low pay, little job security, few fringe benefits and poor working conditions. The term has not caught on, but the notion of the dual labor market, which has been expanded to the more broad concept of stratified labor markets, has become current, particularly through the work of radical-political economists. The stratified labor market approach differs from the traditional demand-supply analysis in that it does not conceive of the labor market as a simple queue in which workers are assigned a ranking in order of their competence, skill and ability and then picked for jobs in priority order. The stratified view contends that there are distinct breaks in the labor market. Some workers have very little chance of ever moving into the prime or mainstream labor market of “good jobs,” even when the demand for labor increases, while others may cross over from the marginal, irregular economy into the mainstream sectors.

The academic labor market is highly stratified, currently into four principal categories: (1) tenured (senior) and tenure-track (junior) “regular” faculty; (2) multi-year contract (but not tenured) and “soft money” “regular” faculty (“irregular” might be more accurate); (3) “turnover” and “part-time” faculty, i.e., the itinerants and casual labor of the academy; and (4) graduate teaching assistants, an important labor pool of many large undergraduate programs. Initial employment used to be the critical point in academic careers, now tenure has become the crucial event. There are numerous ramifications of this shift in patterns of academic stratification and the related changes in academic careers. In this comment I will necessarily limit my discussion to the issue of part-time work as discussed in the two articles, but the larger context of the academic labor market frames the issues of part-time employment.

The two articles review the negative consequences and characteristics of part-time work, but it is important to realize that not every part-time job inevitably carries with it low income, low fringe benefits and low security. Indeed, in the 1960s when I met many top executives in business, the public sectors and universities, I noticed that the higher the position, the more likely it is to be treated as a part-time activity. “Top people” are on numerous outside committees of one kind or another, place themselves on boards of many organizations, attend many conferences, and in general seek a public role which is little related to the ongoing activity of their businesses or organizations.

Another type of part-time work is illustrated by physicians in hospitals. Most physicians

work in hospitals only part of the time, but they determine much of what takes place there, certainly in regard to their patients. They are able to dominate a hospital even when not fully committed to its activities.

Even many leading academics are essentially part-time. In many universities, the more eminent the scholar, the less time spent on campus. Indeed, people formally defined as part-time may actually teach more than those celebrities who have full-time status.

If it is not the fundamental character of part-time work which leads to difficulties for the part-timer, what are the causes of poor conditions, and what can be done about them?

One problem is that many part-time people mainly teach extension or evening students, who are frequently regarded as inferior to day or residential students. Some university administrators apparently see extension programs as a cheap way to make big money by underpaying a faculty with special, lower status. This is not regarded as exploitative or unfair to the students because they are not regarded as worthy of full-time, appropriately recompensed faculty. Part-time faculty could work with part-time students to improve the conditions of both.

Another factor is the over-supply of part-timers relative to the demand. Universities are able to choose a relative few from a large pool of persons with academic training but limited local opportunities to exert their full talents. I know of one leading university that offered a position to a rather well-known novelist to teach a section of first-year English. When she inquired what she would be paid, she was informed that this elite university customarily did not pay people to teach sections of first-year English since so many people were desirous of having this part-time opportunity. I am glad to report that she refused to be exploited.

One possible way to alleviate the supply and demand imbalance is to expand the demand for faculty in universities. The universities should make an effort to explain the values of higher education that go beyond the possible financial benefits—the ability to absorb cultural activity in the United States, to participate in a variety of institutions, to feel a full citizen. They should also make more of an effort to attract adult students and other nontraditional learners.

Many part-time faculty are women, frequently regarded as secondary wage-earners or as people who somehow do not have an academic career line and consequently can be paid less. Women could organize to raise the issue of part-time workers as a feminist issue. This could be particularly effective with regard to medical and other fringe benefits, since many

universities claim that they would be willing to provide medical coverage to part-timers, but their insurance carriers prevent them from doing so. This would seem to be an issue of discrimination against part-time workers, and particularly women.

In the past, and even today, many people looked on part-time academics as apprentices who could be paid an apprenticeship wage, or as people with full-time jobs who thus didn't need the same wage as a person dependent on only one salary. As the figures presented by Tuckman, Caldwell and Vogler make clear, this view is no longer valid.

Where there are unions representing faculty, there should be a special emphasis on the situation of part-time people, particularly where part-time faculty provide a high percentage of faculty time. On the other hand, it cannot be expected that full-time faculty, many of whom feel threatened by the advance of their part-time colleagues, will be willing to fight for improved conditions. It is important that part-time faculty organize themselves into some sort of pressure group to do something about their own conditions.

I think it is an obligation of graduate departments to indicate to their students what their employment prospects are. But I do not believe universities should prevent able students from going on to graduate training if they wish to do so. Graduate schools should admit students to the extent that they can adequately train them, and should inform the students about their prospects. It is up to the student to decide what to do.

Graduate departments should play a much more active role in placing their students than they have in the past. One way they could do this is to directly confront the prevalent notion that if one is engaged in an activity only part-time, then that activity is somehow not "worthwhile." If part-time work in general were more acceptable as a life-style, then new faculty arrangements such as de-tenuring, early retirement, part-time arrangements and sharing of work could be widely used to the benefit of the presently under- or unemployed.

I do not wish to downplay the plight of part-time academics by pointing out the larger scene in which their situation unfolds, but I think it is unlikely that they will see much improvement until universities and faculties begin to grapple much more thoughtfully with the situation of the 1970s and 1980s. I think faculties have been slow to recognize the changes that universities and colleges face. In responding to this situation we must always make sure that those at the bottom do not suffer in order to protect those who are somewhat better off. Part-timers are likely to get the short end of a very dirty

stick if there is not some improvement in the conditions and prospects of universities and colleges themselves.

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1968 "Malemployment: The problem of underpaid and dirty work." *New Generation* 50:15-18.

The papers by Tuckman, Caldwell and Vogler and by Van Arsdale provide a poignant contrast between an overall view of part-time academic employment and its meaning to an individual "Hopeful Full-Timer" (HFT). It is the contrast between a map of an entire battle-zone and the experience of a single frontline soldier.

There is also, however, the organization's perspective on the topic. As the elected chairperson of a department for the past nine years, perhaps I can complement these two papers with a brief discussion of the "institutional imperatives" involved.

First of all, enrollments can never be predicted with complete accuracy, but it is always important that full-time faculty teach their full loads. Neither burdening some members with extra students when enrollments are higher than anticipated nor reducing some members' loads when enrollments are unexpectedly low is an acceptable solution, for obvious reasons. Beyond certain limits, then, flexibility is possible only through the ad hoc use of part-timers, and this has been true even in the best of times.

In the past, it was ordinarily assumed that people sought part-time academic posts either in addition to full-time jobs elsewhere or to support themselves while in graduate school. In neither case was part-timing considered a "problem." Today, though, the combination of straitened academic budgets and an oversupply of would-be academics has produced a situation in which not only are more people hired on a part-time basis but more of them, like Van Arsdale, are Hopeful Full-Timers.

From a hard-nosed institutional point of view, this situation is a problem only as it may injure the institution's interests. Unfortunately, but logically, concern for the economic injustices and low status inflicted upon the part-timer is a luxury when the institution's budget is in trouble. Yet its long-run interests

are injured by the presence of more part-time instructors: the extra dedication to the job that is expected of full-time members of the institution is likely to be missing in those who work part-time for it, and certainly its scholarly reputation suffers because part-timers are less motivated or able to engage in research and writing.

But it is the short-run situation that concerns us here, and I am inclined to agree with most of the rather gloomy assessments of Tuckman, Caldwell and Vogler that little can be done to improve things in the near future. For instance, even without their current fiscal woes, it is clear that academic institutions could not justifiably give full-time appointments to all those qualified professionals who are seeking them. The choice, then, seems really to be between giving *some* HFT's full-time appointments (thereby cutting off a large number from *any* connection with academia) and making part-time connections available to as many unemployed academics as possible. Economic considerations at present seem to dictate the latter course, and in my experience most HFT's prefer half an appointment to none at all.

Given the inevitability of an increasing number of part-timers, whether or not they are HFT's, how much room for maneuver do we have in trying to alleviate the economic and status deprivations they now encounter? Unhappily, the answer seems to be, "Not very much."

With respect to remuneration, it is obvious that if part-timers were to be paid proportionately at the same rate as full-timers, academic institutions would quickly limit their use of part-timers to the minimum needed for flexibility in scheduling, on the principle that full-timers are always preferable to part-timers. A much larger number of HFT's would thus be deprived of all connections with academe, and for this reason I disagree with the recommendation of Tuckman, Caldwell and Vogler that efforts be made to achieve proportionate pay.

(It is particularly at this point that the interests of specific academic institutions and those of the academic profession as a collectivity come into conflict. I am opting for meeting the needs of the latter on the ground that while it is not the ideal solution, it is more merciful to provide some opportunity for the exercise of professional skills than none at all.)

If the economic disparities between part- and full-timers cannot (and under present circumstances should not) be eliminated, what are the prospects for minimizing the status-deprivations that are of such painful concern to Van Arsdale and other HFT's? Here too, I fear that the logic of organization works against

their wishes. Intrinsic to part-time work is the assumption that it is ad hoc, which means that no deep or long-range commitment to the relationship is expected of either party. This shows up, as Van Arsdale shows us, in the careful—and bluntly impersonal—wording of part-timers' contracts. I suggest, though, that this stems principally from academic institutions' understandable dislike of lawsuits and union grievances rather than from any deliberate attempt to belittle their part-time employees.

Whenever office-space or secretarial assistance is inadequate, it is inevitable, by the same logic, that part-timers will be allotted less than full-timers. (I must say, however, that the situation described by Van Arsdale seems an unnecessarily extreme case.) It will be difficult even to make part-timers' relations with full-timers less invidious, since informal relations within a department tend to be shaped by differences in rights and responsibilities.

To conclude this not-very-optimistic comment, I should point out that concern for the problems of academic part-timers is essentially an acute symptom of a larger problem—the present oversupply of professional academics. It is important that we keep this context in mind when seeking remedies.

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Juxtaposed with the declining academic labor market is a redistribution of talent occurring in all academic disciplines. This redistribution is taking various forms: sequential postdoctorates, shared and joint appointments, collective tenure tracking, long-term adjunct positions, specialized institutes removed from one's departmental affiliation, early and extended retirements, and part-time employment. In the profession of Sociology, employment problems are particularly acute and measureable. In spite of market changes relative to need and hence demand, sociologists cling to an endemic status differentiation perspective which places a lower value on perhaps their potential salvation: the nonacademic sector (*ASA Footnotes*, May, 1978). This perspective, if sustained, may be detrimental to the survival of the discipline and the profession.

While issues with which Tuckman, Caldwell and Vogler, and Van Arsdale deal reflect the general labor market picture with respect to part-time employment, there is a pervasive academic consciousness inherent in these pre-

sentations. Tuckman and his coauthors begin with the recognition that opportunities for academics to move upward or even laterally are decreasing as tight budgets have an impact on academic labor markets. In their analysis of data from a national study of part-time faculty, they point to structural problems part-timers face, such as positional location in the rank structure, workloads, hours, fringe benefits and salaries. They then construct a typology, *not a taxonomy* as they state, of academic part-timers. Van Arsdale, on the other hand, reveals a psychological and symbolic portrait of the part-timers' situation. His narrative is emotionally moving as well as informative. Tuckman's analysis is factually instructive.

However, one problem with these presentations is a preoccupation with academe. Both discussions focus on academics and exclude a concern with increasing numbers of full- and part-time professionals outside of academia (*ASA Footnotes*, April, 1978). The orientation of Tuckman, Caldwell and Vogler is especially representative of this prevalent stance (Panian and DeFleur, 1974:8–9). One example is their comment that the transition toward part-time faculty in junior college markets may have affected demand for full-time faculty, "perhaps to the detriment of new PhDs seeking full-time jobs at these institutions" (p. 185). The pertinent question here is: Why should new PhDs frantically search for work in a tight and highly competitive arena as well as a declining market? Should not sociologists, other scientists and those in the humanities, be equipped for employment in nontraditional work settings? Otto Larsen, former Executive Officer of the American Sociological Association, aptly observed a few years ago that "the implication is clear: academic sociologists must divest themselves of certain myths if they are to meet their training obligations or pursue opportunities for non-academic careers themselves" (Panian and DeFleur, 1974:1).

It is interesting that within the federal government, the absence of part-time options is viewed as a form of de facto discrimination against working mothers since it denies many access to nontraditional and better paying career opportunities. "The rigidity of the traditional 9 to 5 workday adds an unnecessary burden to the already delicate fabric of American family life" (*WEAL Report*, 1977:1). In fact, legislation was introduced in 1977 to provide increased employment opportunities in federal agencies for persons unable to work standard hours. Testimony presented for a coalition of women's groups included the following comment:

More part-time jobs at advanced levels would give women a chance to make fuller

use of the education and skills that are now being wasted on jobs that do not begin to tap their talents." (*WEAL Report*, 1977:6)

In a tight job market, the academic bias, which takes a dim view not only of part-time employment for men and women but of nonacademic work as well, is a serious impediment to the growth and broader marketability of a number of professions. The critical problem is not the status of part-time employees but for sociology, at least, unemployment and underutilization of sociological skills. However, if the "skills" sociologists have are not presently in demand, then graduate training programs must be redesigned. Graduate and undergraduate departments must begin to bring career education into the academic classroom. If functional programmatic changes do not occur, there may be no place for either those entrenched in the academy or for neophytes clamoring to get in (Orzack, 1974; Manderscheid, 1978; *ASA Footnotes*, August, 1978; Wilkinson, 1978).

How to prepare for nonacademic work was well articulated in Panian and DeFleur's analysis of *Sociologists in Non-Academic Employment* (1974). Their points are worth mentioning here as a summary of this commentary:

1. Non-academic sociologists need more training in methods, data analysis, statistics, and mathematics.
2. Non-academic sociologists need a broader conceptual framework within which to work. This may mean more course work in other departments; a reduction in the number of areas in which a student may specialize; and the requirement of specialization in general sociology. (See *ASA Footnotes*, March, 1978.)
3. More attention should be given to practical skills—proposal writing, writing and editing, small group leadership, and policy formation. (See Wilkinson, 1977.)
4. Research experience should be provided, either through intern programs or developing research capabilities within the department. (See *ASA Footnotes*, February, 1978.)
5. Theory courses should be modified to include practical consequences. (See *ASA Footnotes*, January, 1978.)

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REJOINDER

The editors deserve praise for assembling a spectrum of views. Unfortunately, several points seem to have slipped through an otherwise well-spread net and we welcome this opportunity to bring them back for inspection. A single labor market does not exist for academics, either full- or part-time. David Brown laid that myth to rest in the 1960s (Brown, 1965), yet some academics still fail to recognize the existence of separate markets. Our earlier analysis of the part-time market (Tuckman and Vogler, 1978) discussed differences among two-year, four-year and university institutions. We also discussed the single (or limited) buyer conditions that exist in what is essentially a set of local labor markets for part-timers.

Professor Ewer recognizes local differences in her comments but many of the other commenters do not. In the Kraft comment, the failure to recognize that part-timers face different supply and demand conditions in different markets leads to the conclusion the oversupply of PhD job seekers can be taken care of by the growth in the number of part-time positions. We disagree. An econometric model is cur-

rently being constructed to estimate the extent of such displacement, if any. In the absence of such a model, we believe that it is premature to use aggregate data on positions to suggest that the massive growth in part-time employment at two-year schools is responsible for a retrenchment of full-timers at other types of institutions. Comparatively new PhDs have been hired at two-year schools, even in times when faculty have been abundant. And if hirings of part-timers at universities alone are considered, the growth in the number of part-timers is hardly sufficient to account for the reduction in the number of full-time positions. Kraft's observations are interesting but, at best, conjectural.

A second potentially misleading assumption growing out of a single market model is that the reasons for hiring part-timers and their employment conditions are the same across institutions. In certain cases (for example, evening, adult, and/or continuing education programs) persons are hired who are easily replaceable and who have relatively "limited" skills. In this clearly monopsonistic situation, the deplorable conditions cited by Van Arsdale, Ewer and Karmen can flourish. In contrast, at other institutions (e.g., Medical Schools) highly specialized part-timers are hired. Persons in the latter category are less likely to fit Kraft's edp analogy (p. 208) or to take the jobs of full-timers as suggested by Ewer. The solutions (compromises?) of the eighties are more likely to be suited to the particular local situations faced by part-timers than to be based on a single general model.

At issue also is which economic theory ought to be used to characterize the labor market for academic part-timers. Deutsch faults us for ignoring "the structural, economic and behavioral causes and dynamics of the problem of part-time employment" (p. 202). Apparently, he would have us focus on a *system* which permits double-digit unemployment rates for subgroups of its population, rather than on the single-industry monopsonistic framework in our earlier work. While we agree on the need for studies of the structural composition of the economy and on the need for analysis of the effect of structure on economic outcome, we disagree that this symposium is the place for such a study. We also wonder whether such a discussion would yield useful insights on the part-time question. We have already noted that institutions rather than personal characteristics determine part-timers' salaries (Tuckman and Caldwell, 1978) and that institutions have considerable power over working conditions (Tuckman and Vogler, 1978). A more specific analysis of the impact of this power would not emphasize economic but rather social and psychological factors.

Presumably, had we followed Deutsch's dic-

tum, an analysis of "sexism," "exploitation," and perhaps selfishness would have followed. But what is exploitation and selfishness to some is a sharing of academe to others. For Storer, the growth in part-time positions provides an opportunity for those denied entrance to the green on a full-time basis to teach at least part of the time. It is worth reiterating that persons have different motives for becoming part-time and that the Semi-Retireds, Full and Part-Mooners and Homeworkers report they are essentially a happy lot. For some, the alternatives to a part-time job in academe are even less palatable than the "exploitative" situation that several of the commenters allude to.

Although employers of academic part-timers may have the upper hand in setting down employment conditions, Wilkinson correctly points out that there are nonacademic alternatives available to discontent part-timers. One analysis (Tuckman, 1978) suggests that of all part-timers who sought an academic offer about 33% received one; of those seeking a nonacademic offer 83% received one. But there are limits to the nonacademic "escape" route. HFT's were the least likely to receive either an academic (13%) or nonacademic (41%) offer. Their plight surely calls for remedial action and our recommendations provide one departure point.

Goldman's emphasis on marginalization recalls our own earlier efforts to fit a dual or segmented labor market model to the part-time data. The shoe pinched too tightly. As a group, academic part-timers are better educated and wealthier than other part-timers. Many are marginal to academe in Goldman's sense (a few are not), and almost none are marginal in the Piore-Doeringer sense. Goldman's marginalization is surely likely to have different consequences where part-timers are used as revenue-raisers than where their use raises the quality of educational offerings. He ignores this distinction.

Goldman also raises several definitional points. Our HFT category includes persons who hold either two or more part-time jobs or a second full-time job. Given the hierarchical selection used, this follows because the HFT category comes before the other ones. We reasoned that the person's wish to be a full-time academic predominates over the fact that he/she might have a second part- or full-time job. Our data include persons on one-year or nonrenewable contracts but only if they teach less than 100% time during the academic year. They exclude full-timers who for various reasons opt for a part-time load. Like Goldman, we exclude students since a large proportion are likely to become HFT but are not ready to achieve this status.

Commenters Macke, Miller, and Deutsch

allude to sexism. Our data do not provide statistical confirmation of discrimination against women either in wage or fringe programs. In fact, they suggest a reverse discrimination in salaries. A paper currently underway will provide information on differences among the sexes in course assignments, etc. At this stage we are loathe to go beyond what has already been said.

Storer's comments provide a sharp counterpoint to those of some full-timers who call for a cap on the number of part-timers. Ignoring the negative effects of Goldman's marginalization, Storer emphasizes the positive value of making at least a piece of academe available. While we doubt that this view will ultimately prevail, it, and its concomitant which stresses the positive effects of new, albeit temporary blood, deserves public airing. One wonders though whether the choice has the same meaning at the different types of institutions.

The use of part-timers has both positive and negative aspects. The challenge is to minimize the negatives and to utilize part-timers as a means for offsetting the worst effects likely to be experienced in the period in front of us.

Howard P. Tuckman
Jaime S. Caldwell
William D. Vogler

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1965 *The Market for College Teachers*. Chapel Hill: The University North Carolina Press.

REJOINDER

Kraft, Deutsch, and Goldman each express important reservations about my lament for professionalism lost: the failure to provide a more sociological analysis, and the narrow focus on the daily conditions confronting part-time faculty members. However, their reservations trouble me less than the excessive significance other readers attach to the simple fact of a "present oversupply of professional academics" (Storer, p. 212).

My choice of the term "de-professionalizing" to describe these conditions arose in part from a belief that no other term would buzz so threateningly in the ears of TAS's principal audience: full-time academic sociologists. The word might help these readers recognize their essential community of interest with their part-time colleagues who are caught by rising tides in the pool of Goldman's "marginal academics." I sought to prick professional conscience, but I accept Kraft's reproach and share his lack of sympathy for those who hide behind their "professionalism" when cost-effectiveness looms and the Dean

pinches their perks. However, when more academics realize that their professional privilege hastens their becoming a powerless elite, they may begin to favor the remedies implicit in Kraft's and Deutsch's final paragraphs and explicit in the recommendations of Karmen, and Tuckman, Caldwell and Vogler. If neither Deutsch nor Goldman seems very sanguine that these remedies will retard what Goldman calls the "process of marginalization," perhaps it is because they recognize that the "present oversupply" is not the simple villain others have thought it to be.

Oversupply itself does not bear a simple meaning: it is as much a symptom of the need to restructure the microeconomy of academic labor as it is a cause of that more alarming restructuring (partially discerned by Tuckman and colleagues) already occurring throughout higher education.

Oversupply has permitted administrations to produce and consolidate major cost-reductions, partially by increasing their draft from the pool of marginal academic labor. As Kraft points out, it is naive to think that the measures proving so effective during this period of oversupply will be forgotten should supply again be brought more into balance with demand: These measures hold rich promise of further cost-benefits if they can be applied more widely to such other labor intensive sectors of higher education as the full-time faculties. For whether in oversupply or not, faculty labor remains the largest cost itemized under "Instruction" in institutional budgets. Therefore it must always appear to responsible academic managers, guided by objectives and cost accountants, as a plum ripe for squeezing. Thus, the restructuring of higher education is unlikely to restrict overproduction of academic labor (one of the chief products) as quickly as it achieves the greater production efficiencies resulting from Karmen's "invasion of scientific management practices" (p. 207). If oversupply has aided the "proletarianization of part-timers," the reduced instructional costs of their extended use must only inspire administrators to plan the more miraculous transformation of full-timers into great gaggles of learned geese who lay golden eggs but consume little corn.

The alarm expressed about the oversupply of academic labor deflects attention from the fact that administration is also a growing high cost budget item. That middle and upper echelons of higher education management have recently grown far more labor intensive is no less remarkable than the impact of oversupply on part-time sector growth. It is not frivolous to suggest that the labor cost of administrative growth is generally being paid for by reducing the labor cost of instruction. It would seem more sensible for faculty members—full- or

part-time—to be alarmed by the “present oversupply” in the administrative sectors of higher education. There a truly new sort of academic professional gains strength daily: the very expensive manager, employed chiefly to reduce costs elsewhere.

Goldman’s call for more attention to those variables that point toward full-time or part-time employment such as race, sex, parent’s education and occupation, and individual training must await further research, but I am certainly not indifferent to the egregious effect on women, denounced by Macke, Miller, and Wilkinson among others, the exploitation of part-timers has often had. Moreover, I have often thought that my own lower middle class background, a feature I share with many of my part-time colleagues, significantly limited the

success of the upward class shift my educational attainments would normally entail. In my extended family, mine is the first generation to acquire even undergraduate degrees; I am the only one who has sought to become a professional academic. My shortfall has, among other results, enhanced my class consciousness and made me deeply suspicious of Storer’s belief that “it is more merciful to provide some opportunity for the exercise of professional skills than none at all” (p. 211). I tend rather to think, with Deutsch, that such mercies betoken the cunning of academic managers and manpower specialists, indifferent to the claims of justice or human needs, hell-bent on cost-reduction with minimal systemic restructuring.

George Van Arsdale

THE MYTH OF NONACADEMIC EMPLOYMENT: OBSERVATIONS ON THE GROWTH OF AN IDEOLOGY*

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There is a belief gaining wide currency in some academic circles that the emphasis in PhD training programs should be shifted away from the traditional preparation for scholarly roles within the academy toward preparation for other sorts of careers outside of colleges and universities. This belief is currently achieving considerable popularity in the field of anthropology in particular, as evidenced by the fact that it has already attracted several slogans, some pre-existing with different meanings—for example, action anthropology, innovation, and diversification—in addition to the increasingly ubiquitous if more prosaic “nonacademic employment.” I cannot speak for other disciplines regarding the popularity of this belief and concern, thus I confine my remarks to anthropology. I will argue that the growth in anthropology of the current enthusiasm about nonacademic employment is a paradigmatic example of the growth of an ideology in

Marx’s sense. To the extent that other academic disciplines share the belief, the argument applies to them as well.

A belief or system of beliefs constitutes an ideology for Marx if: (a) it has adherence among both the powerful and the powerless; (b) it is in important ways false; and (c) its acceptance tends to maintain the status quo—i.e., tends to work to the advantage of those currently powerful. A homey example is the contention of contemporary American Marxists that the belief that anyone with brains can get ahead by hard work is: (a) widely held in various strata of our society; (b) false; and (c) advantageous for the capitalists because it discourages the noncapitalists from revolting. The example is offered not as an original piece of social analysis, or even one I fully accept, but as an illustration of Marx’s notion of ideology.

Is the growing enthusiasm among anthropologists for the future of nonacademic employment: (a) widespread in that profession; (b) without serious empirical foundation; and (c) advantageous for those who hold power in the profession, i.e., tenured academics? I think the answer to each of these questions is Yes.

* Reprinted with permission from *Anthropology Newsletter* 18 (Oct., 1977) 11–12. [Address all communications to: Paul Kay, Dept. of Anthropology, University of California, Berkeley, CA 94720.]

With regard to question (a), argument beyond that already given would appear unnecessary. Few, if any, anthropologists will disagree that that field is currently witnessing a burgeoning enthusiasm for nonacademic employment and for correlative intellectual concerns—if they may be properly so called. (Nonanthropologists may check this claim with the nearest anthropologist.) Affirmative answers to questions (b) and (c) will take only a little longer to establish.

Turning to question (b), what is the likely future of an increase in nonacademic job opportunities for anthropologists sufficient to absorb a significant fraction of the PhDs for which no academic jobs are available? A very quick rehearsal of the well known and frequently discussed sources of the present and projected academic job shortage will take us most of the way to an answer. On the supply side, the academic establishment is continuing to turn out PhDs at a rate that was reasonable in the sputnik era of the early and middle 60s, when academia appeared to be growing at a constant and reliable rate into the indefinite (rose) future. Because of the World War II baby boom, college enrollments were rising, increasing the demand for one sort of professorial service, teaching, while Federal and State governments and private foundations were enjoying the tax revenues of a booming economy and pouring money into research, motivated in significant part by the notion of cold war competition with the Soviets, and creating heavy demand pressure for the other major professorial service, research. As is too well known to bear repeating, both the enrollments and the money are now decreasing. Enrollments will diminish further in the coming years and the best anyone in the academy can hope for in the face of the decreased demand for professorial services is sufficient budgetary support to keep the present holders of the PhD employed in academia, much less those to be produced in succeeding years (D'Andrade, Hammel, Adkins and McDaniel, 1975).

That job opportunities for anthropologists within the academy are shrinking rapidly is well realized. What is apparently hardly realized at all is that the same economic forces that militate for the

shrinking of job opportunities within the academy militate equally for the shrinking of job opportunities for anthropologists outside the academy. In the economy as a whole, the ratio of available jobs to members of the labor force is not increasing and may well be decreasing. The US is probably in for a considerable period of less than full employment. There are not now and by all indications there will not be in the foreseeable future, new jobs outside of the academy for anthropologists to step into. That means that anthropologists who carve careers for themselves outside of academia are going to have to take their jobs away from somebody else: social workers, lawyers, doctors, politicians, public health workers and so on. Leaving aside the complex ethical questions raised by the idea of training anthropologists to take away the jobs of other professionals—or nonprofessionals—it seems unlikely that the current occupants of these jobs will yield them without a struggle. Nor does it appear likely that anthropologists with incidental training in law, medicine, policy making, social welfare, public health and so on, will be able to compete successfully in large numbers for jobs traditionally held by specialists in those fields. I am not saying, of course, that no single anthropologist will succeed in such competition, only that sufficient numbers of anthropologists will not succeed in wresting jobs from their traditional occupants to prevent serious unemployment of anthropology PhD-holders. The number of jobs in the total labor market is finite. Since we already have considerable unemployment, every nonacademic job an anthropologist gets, someone else loses. Add to this the sobering thought that the new nonacademic anthropologists will be competing not only with the traditional holders of the nonacademic job they seek, but also probably with a cohort of new, action psychologists, action sociologists, action political scientists, etc., not to mention action physicists and other refugees from the overpopulated natural sciences.

In sum, anthropologists who have spoken about expanding nonacademic job opportunities for holders of the PhD in anthropology have not looked realistically at the finite nature of the US labor market. That market will continue to be tight and fiercely competitive. There is no reason to

believe that PhD anthropologists will be able in large numbers to displace the professionals already holding jobs in that tight market, especially when faced with the added competition of refugees from other overpopulated academic disciplines. We must conclude that the hope for a sufficient increase in nonacademic job opportunities for anthropologists to accommodate PhDs produced at the current rate is a false hope.

Turning now to question (c); we ask who stands to gain from the growth within the institution of anthropology of PhD training for nonacademic employment. The answer, brutal but unavoidable, is the occupants of tenured positions in the "leading" universities—those that do most of the training of PhDs. These individuals are paid in large part to train graduate students and if their graduate enrollments fall, their budgetary justification declines apace. If the anthropology department unilaterally decreases graduate enrollment, then the Dean sees a little daylight to fund the new program the sociology department wants to start—perhaps a program in the new action sociology. Graduate students are necessary to the faculties of prestigious institutions in a variety of other ways: graduate students provide research assistance and collaboration; they serve as a sounding board for a faculty member's new research ideas; they enhance their former mentor's reputation—and hence his earning power—when they become professionals and cite his works and assign them in turn to their students. Graduate students are necessary to those who train them in many ways, but above all graduate enrollments are an indispensable pillar of the budgets of graduate-oriented departments. It is in the interest of graduate faculties to have graduate students and the members of those faculties stand to gain from anything that militates against a decrease in graduate enrollments. The myth of nonacademic employment is what saves the graduate faculties from facing their real dilemma: either cut graduate enrollment—and their own financial throats—or, by admitting them in large numbers, exploit graduate students many of whom are surely destined for unemployment as professional anthropologists.

Not only the most powerful people in

anthropology, members of the graduate faculties, but also many of the least powerful professionals, graduate students, have subscribed to the myth. What do the students stand to gain or lose from believing in the chimera of nonacademic employment? The students as a whole will, of course, suffer from the fact of their overproduction. If graduate enrollments are not drastically decreased, there will be massive unemployment of PhD anthropologists in any sort of professional level work. (Whether or not the professionally unemployed individuals will compete successfully for clerical and blue collar jobs, I cannot say. In any case resolution of that question is not germane to this essay.) It seems quite possible that the students trained for nonacademic employment will face greater difficulties in finding jobs than those following a traditional curriculum. There are too many imponderables involved for one to assess that question definitively, but in any case the larger the proportion of anthropology PhDs trained for nonacademic employment the worse will be their job chances relative to their fellow students who follow a traditional academic track inasmuch as the former individuals will have taken themselves off the academic labor market.

Training PhD students for nonacademic employment is rapidly becoming a profitable enterprise for established members of established departments. Concern with nonacademic employment, with various forms of action anthropology, is a burgeoning field within academic anthropology as evidenced by presentations at annual meetings, the kinds of new graduate training programs being funded, space allocation in the journals and the *Anthropology Newsletter* and so on. Ironically, nonacademic anthropology is being vigorously and profitably pursued within the academy by established academics, but there is no evidence that the pursuit of nonacademic anthropology has any important present or likely future existence other than as a convenient ideological prop to the status quo within the academy.

We have seen that there is no empirical support for the belief in a future bonanza of nonacademic employment that will provide jobs for the excess PhDs being produced at current rates. We have also seen that this belief is widespread among

both the powerful and the powerless within the institution and that it served to bolster the status quo by enabling graduate faculties to reconcile their refusal to cut enrollments with their liberal consciences. In short we have seen that this notion is, in Marx's sense, an ideol-

ogy rather than any form of objective knowledge or rational belief.

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COMMENTS

Whether or not Kay's analysis of myths and vested interests within anthropology can be extended to sociology, it does appear that we may be heading for disaster unless we begin to restrict the number of new PhDs turned out each year. The last figures I saw indicated that we are currently producing somewhat more than 700 doctorates each year, whereas estimates I saw of the net number of new openings in four-year colleges and universities were decreasing to 200 and may even be below that point.

Clearly, it is convenient to believe that with just a little more effort and redirection we will be able to tap some nonacademic or applied market "out there" somewhere. I rather suspect that most departments are presently taking the stance that it is the *other* departments that should be cutting back their PhD productions or instituting applied programs. As long as a department is able to place its own students, albeit less satisfactorily than a decade ago, there will be few incentives to modify drastically our training programs or to seek out these alternative sources of employment. I also rather suspect that very few of us in academic settings are sufficiently familiar with whatever nonacademic employment networks there may be to make this shift a viable alternative. Hence, we have taken much more of an ostrich-like stance than an active or crusading one.

Frankly, I have never understood how we could ever train students in anything as broad as "applied sociology," since I do not know what it is! I *can* imagine future PhDs working in applied areas of demography, criminology, or even race relations because these are much more restricted subfields. If we can locate positions that tap sociological expertise in research methods and/or specific subfields, then I think we would be foolish not to encourage our students to fill these positions, and indeed I would contend that the world would be much better off for it. It has become clear to me, from my own limited consulting experience, that there are many incompetent and poorly trained persons in state and federal governmental positions who *ought* to be replaced. At least, I am quite certain that many of them know next to

nothing about research. I share Kay's pessimism, however—it will be difficult to dislodge them or even to persuade policy makers that sociologists are in fact better trained. But I think we should continue to make every effort to do so, and to place students in whatever applied openings happen to turn up. Furthermore, we need to communicate to students that applied research is an o.k. thing to do.

Having said this, let me express two concerns about *existing* types of applied research with which I am becoming increasingly familiar. My basic fear is that "applied research" may come to mean "low quality, quick and dirty" contract research. Of course, this need not be the case, even with respect to contract research. But given the backgrounds of many screening committees and the rather marginal commitments of policy-makers to the subtleties, inherent limitations, and methodological concerns of social research, I fear the worst! I am familiar with a number of instances in our state, for example, where persons with *no* training whatsoever in research methods were sent to evaluate research in public agencies. I have also sat in on discussions where it was pointed out that this or that proposal, even though nonsensical, would be supported for what amounted to political reasons. If we had to adjust our training programs in methodology to accommodate these kinds of situations, the impact on standards and intellectual integrity would be disastrous.

My second concern about a heavy emphasis on applied research is, likewise, not an *inherent* problem with applied work, but a pragmatic matter that stems from current practice. I am concerned that many kinds of research sponsored by "outsiders" will detract from the cumulative development of theory. Whereas applied research can be an excellent corrective against the development of overly abstract theorizing, and—when skillfully done—can lead to important theoretical insights, what I fear is the tendency for researchers to scramble about from one contract to the next without ever having to face up to the theoretical implications of their research. In other words, money and those who control it may determine the directions in which we are heading. It

seems to me that perhaps the best corrective for this possible tendency is *more*, rather than less, theoretical training in our graduate curricula.

Thus I do not see an emphasis on applied sociology as an inherent or necessary threat, provided that we can take the necessary steps to instill in our students high professional standards for the quality of research and concern for the generalizability of our findings and their contributions to the intellectual enterprise. However, if we as role models display too great a concern for grantsmanship, and if we bend too much in the direction of conducting quick research that has little or no bearing on larger theoretical issues, then I would be deeply concerned about the implications of sending more than a handful of our students into the nonacademic job market. This is completely aside from the question of the size of the market and our ability to compete in it.

It would be nice if we could be sure that the best trained persons will win out in the competition for applied research contracts, but I have grown a bit cynical about this. Therefore, over the long haul, perhaps one of the most important things our profession and the ASA can do is to convince those who control the purse strings that if they act so as to solicit low quality research, that is exactly what they will get. Meanwhile, it is crucial that each department take a close look at its own admissions practices and academic standards with a view to emphasizing quality and reducing the total number of doctorates by at least twenty per cent.

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While I obviously share Paul Kay's concern over the growing army of unemployed and underemployed PhDs who, hollow-eyed and disbelieving, will haunt the groves of academe for years to come, I do not believe this problem is best understood in terms of Marx's theory of ideology applied to an academic "ruling class." Certainly, the notion of nonacademic employment works in the interests of tenured graduate faculties; as I have written elsewhere, "... the jobs, salaries, power and prestige of faculty in the universities depend on keeping the raw material [i.e., students] flowing through" (Blumberg, 1978), and the idea that nonacademic employment is available encourages that flow.

Yet Kay's explanation of our current predicament is faulty on two counts. First, as every analysis of ruling-class ideology, unless handled with care it easily slides into simple theoretical paranoia. For example, because

the ideology of nonacademic employment happens to service the interests of established faculties, it is a short step from there—and Kay tends in that direction—to the view that the entire problem of the overproduction of PhDs is perpetuated by a conspiracy of tenured faculty consciously concocting ways to keep graduate enrollment high. Although this gratifies our need to find villains, it oversimplifies reality. The notion of the possibility of nonacademic employment is not so much a cynical creation of tenured faculty members to enhance their own positions as it is a hopeful attempt to remedy what is becoming a hopeless job market for academic employment among young PhDs in so many disciplines. At most, there is an "elective affinity," to use Weber's phrase, between the idea of nonacademic employment and the interests of tenured faculty, which tends to make established professors complacent about the career prospects of their graduates. No, our problems have less to do with conspiracy and ideology than with structural defects of the system, ignorance and indifference—which lead to my second objection to Kay's argument.

In his exclusive emphasis on ideology, Kay ignores more fundamental causes of the problem, some of which, in fact, suggest other Marxian insights.

(1) Just as Marx spoke of an *industrial* reserve army under capitalism, we now face its late 20th century counterpart, an *intellectual* reserve army. Both derive from the same condition—the anarchy of production. In one case, it is the anarchy of commodity production, in the other, the anarchy of educational production. The latter manifests itself as a lack of national planning and coordination between educational institutions on the one hand and the economic institutions on the other, by which the supply of scholars might be intelligently brought into line with economic demand. Laissez faire has again given us not the invisible hand, but anarchy and wasted resources. Note, however, that when we speak today about the overproduction of PhDs, we speak not of overproduction in an absolute sense, but overproduction relative to the ability of the society, as currently organized, to make productive use of its trained workforce. If American society were to begin to dedicate itself to the creation of a truly universal system of post-secondary education, to a "learning society," as the Carnegie Commission called it, there would be an actual shortage, not a glut, of scholars and teachers (Carter, 1976). Just as Marx observed ironically that overproduction—abundance—provokes crisis in a market economy, so the academic crisis today stems from a comparable embarrassment of riches—the irony of a society unable to

utilize its labor force because it is too highly trained, too highly educated.

(2) A related cause of our academic woes is that, aside from Allan Cartter and a handful of others, virtually no educators, administrators or government officials anticipated the current oversupply of PhDs until disaster was nearly upon us. This failure is just part of social science's dreadful postwar prediction record. Demographers failed to anticipate the baby boom and then failed to predict its end; economists failed to foresee the most important economic events of the postwar world—the emergence of the multinational firm, chronic inflation, the rise of Japan and the relative decline of the American economy, growing environmental constraints (Heilbroner, 1973); and many influential sociologists, at the beginning of the 1960s, a decade of unprecedented ideological conflict, proclaimed that we were a society at the end of ideology. The examples could be multiplied endlessly.

Social scientists failed to anticipate the job crisis in higher education—even though the demographic trends took shape years in advance—because they were so caught up in the postwar euphoria of the affluent society, so blinded by what I call the *Time* magazine *Weltanschauung* of onward and upward in every social and economic dimension, so busy partying it up on the *Titanic*, that they naturally assumed that education was a permanent growth industry and that the demand for more schools and more faculty would continue indefinitely.

(3) From a structural point of view, the university is the only factory in modern society that is not responsible for the disposition of its finished products. General Motors cannot merely produce cars and then abandon them in the factory parking lot; they produce only as many as can be sold. Yet universities can turn out PhDs without giving a thought to whether the market can absorb them; graduates can simply be left in the academic parking lot to fend for themselves. This structural fact obviously encourages unlimited production. Universities and their academic departments thus behave a bit like the rocket scientist in Tom Lehrer's celebrated satirical song: "Once the rockets are up/Who cares where they come down?/That's not my department/Says Wernher von Braun."¹

(4) This indifference to the finished product is exacerbated by a kind of professorial snobbery, especially in the humanities, but also in the social sciences, that has traditionally disdained a concern for jobs. Lost in the profundity of Platonic dialogues or pattern variables, professors (with tenure) may regard their

graduate students' mundane preoccupation with jobs and money as crude, gauche and singularly undignified. This has fostered among professors a kind of psychological distancing from the career problems of their graduates, an indifference that C. Wright Mills might have included within the compass of the "higher irresponsibility."

(5) Finally, there is a definite petty bourgeois parochialism about the outlook of the members of many academic departments. Even if aware of the employment problems in their discipline, they may reason that surely their department, turning out only a handful of graduates, could not contribute significantly to the problem. The collective effects of innumerable such decisions are obvious.

While I feel that the issues outlined here are far more important in the continuing academic employment crisis than the ideology of established graduate faculties, Kay's analysis does touch upon a disturbing subterranean theme that is now beginning to bubble to the surface: the growing conflict of generations between older faculty, whose positions are secure, and younger faculty, whose careers are uncertain. Many of the latter have, in fact, developed a counter-ideology of their own—the belief that older tenured faculty owe their jobs more to the date of their birth than to the quality of their preparation and performance. In a profession where the scramble for jobs is on and will intensify during the 1980s, many young PhDs, angry, bitter and jobless, feel that most entrenched older faculty who are now reviewing from on high the hundreds of resumes that cross their desks, could not get the jobs they now comfortably hold if they had to compete for them in today's academic marketplace. Such divisiveness, based on generational antagonism in an atmosphere of bitter competition and job scarcity, will inevitably strain the bonds of consensus in departments, universities, disciplines and in the entire academic profession.

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¹ ©1965 by Tom Lehrer. Used by permission.

It is not clear whether Kay's comments should be responded to in terms of his presentation of a "paradigmatic example of the growth of an ideology in Marx's sense," (p. 216) or in terms of what he apparently sees as a trend that he thinks is an irrational belief and something that should not be. Probably the easiest thing is to deal with both.

First, with regard to Marx's concept: it may fit, but it simply doesn't fit as well as the observation of my mentor, Joe Ziltch, who observed when having difficulty in selling his product, as presumably by implication academics are having difficulty in placing their products, "In this delicatessen, if you can't sell salami, you sell baloney." It is not necessary to elaborate here, but under those circumstances, he would stock more baloney. Each to his own theorist, as we used to say in the old days.

If we really want to know if there is a "burgeoning enthusiasm for nonacademic employment" (p. 217) in anthropology, or elsewhere, there probably are better ways of assessing the situation than asking the "nearest anthropologist," but this may be simply quibbling about research procedures and rules of evidence. I am not convinced that the switch to applied training is that massive in academia, and I am not at all sure that Kay accurately represents the changes that have taken place. Rather, the shift that has gone on and is going on is from a concern with "pure" research, whatever that is, to what is sometimes called "applied" research, and more often is dignified by a euphemism like "policy oriented research." My most distasteful memory of the former is of a colleague who in those golden days of riches in the past said: "I do the research I like, and I don't care if it relates to anything else. Someday it may be important." Somehow even then, I had the feeling that maybe social science research should have some relevance to what was going on in the world; but I was told that much in mathematics was not practical, so why should sociology try to be? On the side of applied research, it seemed that some sources of data could be useful, and some descriptive work of importance could be carried out. This often seemed below the dignity of the academic community, and as one of my colleagues used to say, "Why collect data? All it will do is crap up your theory."

In any event, it is my impression that academia naturally follows the money and the market. Academics simply follow the trend, and so now instead of having 4000 studies a year that repeat themselves on "Body form and physical attractiveness," the popular theme may be the "Symbolic meaning of waiting in an unemployment line"—or some other

"meaningful," policy-oriented or practical topic. Which brings us to the observation of that other great theorist, Joe Pasquale: "If there's a buck in it, someone will do it." With such strong theory behind us, why should we be surprised to see a shift in interest?

Now, a part of that shift in interest may be accompanied by a shift in what is sold, and so with all the money available after World War II, it was no surprise to see the growth of clinical and counseling psychology. No one really asked whether psychologists (or psychiatrists) actually had valid procedures. So, with anthropologists and sociologists, maybe no one asks whether they actually can contribute much to the nonacademic world. But with the abundance of funds available, can any real medical center operate without a team anthropologist or team sociologist, if only for the prestige and presumed authority? So, training people for these teams becomes commonplace in graduate schools.

These changes are not surprising, and neither are a lot of other things that happen in academia. Why should they be? But are they properly represented? First, why should any trained person with a PhD expect to get a job using that training? This is simply one possible way for the system to operate. Further, why should "nonacademic employment" for PhDs need to be generated at all? The idea seems to mean that if people are given training, independently of how they perform they deserve employment using whatever skills they have acquired. This view suggests that people should learn no more than they can use, and that they should not be involved in surplus learning. It is more commonly reflected in the rejection of competition (e.g., as reported in the Ladd-Lipset Survey). There appears to be a no-failure ethos in academia that suggests that no person should have to suffer the indignities of not getting all to which he or she aspires. This is not a new thought, but taking it so seriously seems to be associated with our generation. Many people feel the system should operate quite differently. Possibly competition is not so bad. Possibly people should be encouraged to strive until they fail, and then maybe strive even harder. If the system of knowledge that we ostensibly value is potentially important, then shoving people up the pyramid is important. It is important to have a big pyramid, which means open access at the base, competition, and failure. It may be an ugly system to some, but some may not like the other. Take your choice, if you think you have one.

Kay may be right. There may be no empirical evidence for the belief in a future bonanza of nonacademic employment for excess PhDs. So what? He seems to be complaining that

students are being sold a bill of goods. Maybe, but in comparison to what? I also have a complaint, since I'm not sure I understand the value of the applied pushes in some areas, but that is like saying that the society is less than perfect, or maybe more accurately, *we* are less than perfect. (I had to get this old to discover this???) The applied push seems to me to pervert the idea that we should be training people to be scientists, and instead train them for jobs. This bothers me, but not too much, as the value of scientists is not likely to depreciate too much in the long run.

Response to Kay's position boils down to a question or two. If nonacademic employment for PhDs was not available, should we infer that somehow some other training could be given that would lead to employment? That is, what's the alternative? What could be proposed that would not be an ideology in Kay's sense? After all, any sensible alternative would have support. As a last resort we could turn to Joe Ziltch again, who said: "If you can do nothing, package it well before you sell it."

I think we should pay more attention to training scientists in our wasteful society. That's another ideology. I'm not sure what Kay is proposing. Implicitly, he seems to be proposing that unemployment of *non*-nonacademic PhDs is better than unemployment of nonacademic PhDs. He also appears to ignore the possibility that keeping the trainers employed and the students off the labor market may be part of somebody else's grand design. To note that things are not working right from a particular perspective is trivial. The real mandate is to come up with some alternative that is not "false" and presumably would fit this complex system better.

As for the causal implications in Kay's presentation, the most charitable thing that can be said is that they are naive. Or, to quote Joe Pasquale again: "Nothing's going to change unless something happens."

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We find much to agree with in Kay's essay. While his comments may apply less well to sociology (which has a somewhat stronger tradition of nonacademic employment) than to anthropology, many sociologists seem to expect considerable growth in nonacademic employment, and that expectation serves the same ideological function. We will address three

points related to Kay's article, necessarily omitting discussion of others.

Kay implies that traditional training for academic employment (by contrast with the newer "action" training) is not ideological but rational in character. We disagree. It is important to raise this issue because, in spite of very modest changes in employment patterns, the vast majority of social scientists, even throughout the lean 1970s, did hold academic jobs—79% in 1977 (National Research Council, 1978). How well is the training these individuals receive suited to the work they will actually do?

Training for research in graduate school regularly takes precedence over training for teaching, and yet most college faculty rarely if ever publish the fruits of their scholarship, while presumably all teach (Lewis, 1975). The "lack of fit" between training and work experience will increase as more and more PhDs "bump down" *within* academe, from sociology to non-sociology departments (e.g., criminal justice), and/or from research to teaching institutions where they may find some rewards for competent teaching, but weak institutional and even interpersonal support for their research interests. This is certainly not to say that training for critical analysis and scholarship ought to be abandoned, or that a career in teaching is without value, but rather that graduate education should include attention to the shifting realities of academic employment, as well as to nonacademic opportunities.

Cost-conscious academic administrators, borrowing techniques from industry, attempt to increase "productivity" by encouraging new technologies and new work rules. An extreme example is "telecourses," especially popular in community colleges, which are transforming professors into "course facilitators" who serve primarily to answer questions by telephone about TV-taught courses (Middleton, 1978). Perhaps more commonly, administrators demand increases in class size quotas, and attempt to control the out-of-class time allocation of faculty. Few of us as graduate students anticipated these limitations on our autonomy.

The student population is also changing. Declining literacy and increasing narrow vocationalism among students are much discussed but, it appears, little dealt with in teacher preparation. Declining enrollments create vigorous (if opportunistic) searches for new sources of students on many campuses; nationally, students over age 25 now form 36% of college enrollments (Magarrell, 1978). A wide range of consequences, both pedagogical and institutional, could follow from a changing age distribution (e.g., the concern of older students about a college's convenience may result in

increasing decentralization of teaching and corresponding concentration of administration).

Whose interests are served by ignoring the changing nature of academic employment? Attention to such collective issues as these in higher education would detract from the time and energy available for one's own career development. Graduate faculty, most never trained to teach and some perhaps not very good at it, may avoid having to work on improving their skills. As for preparing graduate students to teach the "nontraditional students" in a nonelite institution, that would be an even greater challenge. Finally, government and foundation funding for teacher development is hardly as abundant as for research. Thus, graduate faculty benefit from ignoring changes within the academy—professionally, personally, and economically. Challenging long-entrenched beliefs and firmly established practices within the academy itself may generate even more resistance than Kay's criticism of the newer "action" approach. Perhaps the ideology that most needs to be explored is the belief in the rationality of graduate training as preparation for academic jobs.

The second point we want to make concerns Kay's contention that high unemployment and job competition are inevitable. This is as much an ideology as the belief in nonacademic employment, and yet he (along with countless others) accepts it uncritically. The unemployment of social scientists must be seen as a small part of our society's continuing failure to employ the talents and capabilities of all of its people, despite the existence of numerous problems in need of solution and human needs left unmet. Unemployment is not inevitable in an absolute sense, although it may indeed be inevitable under capitalism. That, however, is a very different statement from the one Kay asserts. A belief in the inevitability of unemployment functions to defuse discontent and to protect current economic arrangements that benefit corporations and the wealthy at the expense of the majority. As social scientists, it is important that we investigate not only the ideologies of our discipline but also those we share with the larger society.

Finally, just as our analysis must take a broader perspective, solutions to these problems require collective strategies. The ASA should encourage wider participation in directing the development of the discipline, which is now, as Kay suggests, in the hands of the graduate professoriate. The ASA and other groups in the profession should expand their efforts to alleviate unemployment; the SSSP's Pericles Foundation Awards for Research appear to be a modest step toward developing such collective and institutionalized strategies.

Other strategies, such as job-sharing, might be explored. The ASA and its members should join the demand for full employment, not hesitating to take public stands on relevant political issues. In their own work roles, sociologists should support and participate in the development of strong academic unions controlled by their members. Unfortunately, though, faculty at the most prestigious institutions are most likely to oppose faculty unions (Kemerer and Baldrige, 1975:52); by contrast, the objective situation of many college faculties (as we have outlined about) makes unions a necessity in avoiding further erosion of the conditions necessary for good teaching and research, academic freedom, and access to higher education for previously excluded groups. Participation in the union movement would also help social science to see beyond its preoccupation with the needs of various elites—including its own.

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Paul Kay's analysis of the growth of a mythical "enthusiasm for nonacademic employment" (p. 217) in anthropology and other disciplines is tightly argued but adds up, in my judgment, to a countermyth that attacks straw men. He proposes three tests for labeling this as an ideology: (a) that the belief is widely shared; (b) that it is demonstrably false; and (c) that it serves to maintain the status quo. From my sociological vantage point the first is not

true, the second is still an open question, and the third is only a partial truth at best. Let me comment on each of these in turn, drawing upon my recent experience in grappling with the issue of nonacademic employment for sociologists.

First, I have seen very little evidence that the field of sociology is suffused with a "growing enthusiasm" about prospects for jobs outside academia. The ASA's Expanding Employment Opportunities (ExEO) Committee's attempts to define the problem and to suggest useful steps to be taken to deal with it have hardly struck fire either at the grass roots or at the ASA Council level. Skepticism has been expressed as to whether the problem of a declining academic job market is not simply *temporary*, and thus not deserving of curriculum revision, recruiting cutbacks, or some other organized response. Another view frequently encountered is that the problem is substantial but it is part of *larger social trends* not amenable to change or amelioration by mere mortals such as we. Thus, the skeptics wonder whether the problem warrants a reorientation of training and professional values that would make future sociologists more employable elsewhere, while the pessimists doubt that anything can be done about it. Only a few seem to agree that the problem deserves attention, but even among this small group there are some who cannot bring themselves to relinquish the predominantly academic orientation that informs the prime missions and models for training future sociologists. Some enthusiasm!

Kay defends his second point, that the belief is false, mainly by referring to the competitive disadvantage of anthropologists in seeking to supplant others now holding nonacademic jobs or when offering their services at the same time as others displaced by the same market forces. This may be less serious for sociologists, especially those with strong quantitative analytical skills, or who have worked in multidisciplinary problem areas. The potential nonacademic market for such sociologists remains to be tested. Nelson Foote (1974) has long argued that "people problems," in organizations and institutional settings of the most varied kind, provide an agenda of opportunities that can usefully engage many more sociologists than have so far deigned (or been encouraged) to apply themselves to them—other than as part-time consultants who return to their campuses, or as full-time people in parking orbits awaiting academic reentry.

Our ExEO Committee felt that certain steps could be taken to enhance these prospects, based on the experience and expertise of those who have already tested the waters (Gollin, 1977). What seems to be called for is more input from such people, and a coherent,

sharply-focused action research program, regional and national, to define new professional job markets and opportunities that may exist under occupational labels other than "sociologist." A failure of nerve, a belief that sociology really has little to offer, can work to sociologists' disadvantage in seeking to capitalize on such opportunities. But in any case, as recruitment standards rise with the tide of professionalization, it is not at all clear, as Kay would have us believe, that sociologists will be competitively disadvantaged—if they are committed to applying their skills in nontraditional ways and willing to have their work measured by nonacademic yardsticks.

The third point, that graduate faculties propagate the "myth" to save their jobs by holding out illusory hopes to students, may be partially true, but seems to me to reflect an unduly sour perspective on what motivates academic men and women. The goals of teaching, research, and service on which academics are evaluated and rewarded surely represent a broader, more flexible basis for the pursuit of self-interest by most than Kay's formulation admits. Academic norms that have evolved in recent years of job shortages stress "informed consent" as a key aspect of student (including prospective-student) counseling. Few academics could get away with promoting any myths about rosy job prospects for graduate students these days. After all, students read the papers and are part of networks in which career problems and opportunities have a very high priority.

Some may indulge in wishful thinking about the potential academic job market, but this is hardly evidence that powerful and fearful professors are deliberately lying or shading the truth to save their own skins. Yet, this is what Kay would have us believe. His "Marxian" (*pace* Karl!) analysis seems to have led him to see people narrowly, as acting primarily out of economic self-interest. Human motives, including those of academics, are more varied and complex than that. Surely the tenure system protects academics who tell the truth about academic job prospects as much as it enhances the right to find out and publicize inconvenient or unpleasant truths about other sectors of the society. Kay's academics are straw men.

Academics will gain little by turning on one another and in essence blaming those who are also victims of larger social forces such as declining birth rates and inflation. The problem of academic employment is growing more and more serious. Those who care about the future of their disciplines and worry about the career chances confronting our children's generation need to adapt their thinking to the changing circumstances. Kay's analysis does not offer

any suggestions about how this can best be accomplished in our collective interest. *Mea culpa* is not a program.

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My major reservation about Kay's article is the lack of empirical evidence for the more controversial aspects of his argument. No one can prove there will be "enough" jobs, but he cannot prove there will not. Indeed, there is no agreement on what an "appropriate" job for a PhD actually is. It is not clear what is gained by describing the current enthusiasm for nonacademic employment and related graduate programs in anthropology in terms of "ideology in Marx's sense" (p. 216). Although this is never said outright, the article seems to imply that we are experiencing a clever hoax: that somehow it is unethical to train anthropologists for positions other than teaching, both because such positions do not exist and, in case they do exist, because they "belong" to other professionals.

Perhaps the phenomenon to which Kay is referring is better understood as a sincere attempt on the part of many professionals to deal with a projected surplus of doctorates in anthropology. To my knowledge, no anthropologist expects the nonacademic sector of the economy to produce a "bonanza" of jobs for anthropologists as Kay suggests. Rather, based on the experience of anthropologists who have had successful careers outside of academe, attempts are being made to inform and advise students who can no longer expect academic jobs that there are other satisfying and productive careers. Students who enter the more "applied" programs certainly should do so with the knowledge that the market is tight.

The Higher Education Research Institute's (HERI) study of alternative careers for anthropologists has shown that PhDs doing jobs other than teaching can be very satisfied and

well-paid. A related project at HERI shows the same for sociologists (Solomon and Hurwicz, 1978). It is difficult to seriously entertain the notion that there are "complex ethical questions" (p. 217) raised by the intention of training anthropology graduates to be competent in jobs other than teaching anthropology in colleges and universities. Kay's image of interdisciplinary struggle ignores the fact that anthropologists or sociologists will not be hired for jobs traditionally held by other professionals unless the employer feels they are more qualified to do them. One of the most impressive qualities of all the alternatively (here I mean nonfaculty) employed anthropologists to whom I have spoken about their careers is their ingenuity in finding and/or redefining their jobs. The channels for locating academic jobs are well established. Developing the nonacademic uses of our highly trained human resources will take more time.

Let's look at the real figures in question. At present there are only 431 sociologists/anthropologists with PhDs who are working full-time in nonscience positions. While figures are only available for the combined fields, a further breakdown by the National Research Council (NRC) into fine fields indicates that there are nearly three sociologists for every anthropologist. Projections suggest that there will be a surplus of roughly 150,000 PhDs in science and engineering by 1985. If the surplus of sociologists/anthropologists were equal to those fields' current share of doctorate holders in science and engineering (3.2%), the surplus of anthropologists would be 1,200 and of sociologists, 3,600 in 1985 (NSF, 1977). Yet, if each of 1,000 corporations in this country hired five sociologists or anthropologists in the next eight years (that is less than one per year), there would be no surplus. The U.S. Civil Service is another source of jobs for highly trained social scientists who are willing to take them (see Solomon, Ochsner and Hurwicz, 1978, for parallel argument). And, with so many articles on this subject appearing in journals, we need not worry too much about dishonest manipulation of graduate students.

The major problem with Kay's argument is the implication that the only honest course of action is to *prevent* aspiring anthropology graduate students (and those in other fields where academic jobs are equally scarce) from embarking upon graduate study. But who would be excluded: those with low grades or GRE scores, those unable to pay? If restrictions are imposed how will affirmative action be affected? And freedom of choice will be severely limited. The most equitable policy seems to be one of providing the best information possible on job prospects and allowing anyone who still wants to attend graduate

school to do so. Given that policy, any help possible in securing productive jobs should be provided. Those involved in the job search process should realize that bright people can make new types of jobs interesting, satisfying and productive, if they are given a chance.

Kay, of course, offers no direct recommendations; he appears to be content to have "proven" his point. However, this kind of attitude could do much damage if taken too seriously by students desiring to apply sociocultural theory and method in arenas other than the classroom.

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The older I get, the less appealing I find arguments such as Kay's about what must be so—as ever with little or no empirical evidence—followed by a polemical argument about who is to blame—as usual the all-powerful establishment. This argument itself is hardly worth the ammunition needed to shoot it out of the water, but let us deal with it briefly.

Kay's argument, stripped of the Marxian patina, has several parts. First, to use that ugly phrase, there is indeed "a PhD glut." The evidence is all around us that the number of PhDs being produced cannot now be absorbed in higher education. However, the absorption rate varies by discipline. The intake of new graduate students has already declined; demographers are predicting another upswing in the school population in the late 1980s, and many institutions are busily inventing reasons why more and more nontraditional clienteles should come to them for education.

Second, Kay suggests that there is a widespread belief among anthropologists that stressing nonacademic employment will help solve

the problem. He suggested that I check this with the nearest anthropologist. I did. I found a general belief that training for nonacademic employment would help, but hardly solve the problem. Some felt that the field should have emphasized nonacademic alternatives long before the market crisis appeared. My colleagues also mentioned the curious case of what is being called "contract anthropology," wherein archaeologists of all people are now being sought after because in some states construction contractors are required to certify that there are no dinosaurs or other ancestors on a site before they excavate to lay a foundation for a building. Perhaps sociologists can look for the bones of small groups.

Third, Kay argues that a "finite" labor force means that any expansion in employment for anthropologists comes out of the opportunity pool for other professionals. Kay confuses finite with fixed; the problem is relative growth rates, not absolute size and, in any event, it is fruitless to argue about a tiny section of the job market in terms of the characteristics of the total market. What Kay's argument highlights is that we know very little about the details of the PhD market in different disciplines or subdivisions of disciplines. We do not even have aggregate data on the disciplinary distribution of the faculty of colleges and universities, let alone the numbers and employment histories of different kinds of graduates. For instance, I suspect, but cannot prove, that amidst the debate over the placement of new PhD entrants in the labor force, an equally important market dislocation is now occurring almost unnoticed. The crisis in university finances is turning the tenure line into an almost insurmountable barrier, and a substantial part of a whole generation of young scholars is being dumped back onto the market with the added stigma of having been rejected for tenure at their first post. I also suspect that while the superstar market is still thriving, the mobility of those in mid-career has slowed down considerably. But no one really knows.

I would like to put aside as untested at best Kay's fourth proposition that the rise of the "false ideology" of nonacademic employment is a by-product of the tenured faculty's desperate attempt to keep graduate students. The only real argument for this proposition is the fact that the tenured faculty gain from the belief; it does not necessarily follow that they must be both responsible for its growth and corrupt in their motives. To illustrate the shaky ground for Kay's surmise, I could put forth the less plausible but equally logical argument that the villains are really the nontenured faculty, who want to keep the senior faculty busy with the teaching of graduate students so that the undergraduate enrollments will be available to

sustain their own job justification. After all, it is the junior faculty that are most vulnerable.

But leaving Kay's polemics aside, what does the evidence show about the comparative advantage in the job market of people with applied skills over those for whom the nonacademic market seems least promising? At Pennsylvania, we are in the midst of a review of each of our academic departments. We have examined the placement of all PhDs over the past five years, so that we have some idea of the relative placement prospects in a variety of disciplines. Not surprisingly, the least applied, high productivity humanities departments suffer most—Romance Languages, History, English. This seems to be true nationally as well as locally. As Coughlin (1977) indicates in the *Chronicle of Higher Education*, many humanities departments are responding with a series of major efforts to facilitate their students' pursuit of nonacademic jobs. The amount of retooling, special advising, providing supplemental skills, and reorienting going on is truly remarkable. There is some evidence that these experiments have been at least partially successful, but whether this is a durable market strategy remains to be seen. Our survey shows that fields with already established substantial nonacademic markets to supplement their academic placement—natural sciences, economics, psychology—seem to do all right. Anthropology behaves somewhat like a humanities department. But our sociology department has two well-established specialties that have traditionally placed many students in nonacademic markets—criminology and demography. Those graduates have not only gotten more nonacademic jobs; when they are placed at universities, they tend to go to higher quality institutions too, although this may merely reflect their relative strength here. To really answer Kay's question, we need similar information on a variety of institutions. We also need to be able to answer the more difficult question of whether new markets can be opened even if they are deliberately cultivated, and whether these markets are elastic enough to absorb all or a substantial portion of the current surplus. For all the thrashing around in Kay's article, he does not really know the answers to those questions, nor, in fairness to him, do the people urging the pursuit of nonacademic appointment as a placement strategy.

I would like to comment on two aspects of the current situation, one general and one particular to sociology. First, the changed market situation is producing a major change in the nature of the relationship between professors and students. In the recent past, students applied for admission to graduate school, the faculty screened them for quality, and

accepted them. Once accepted, students studied full-time while financial support was provided by the institution, which was responsible for placing graduates in jobs relevant to their areas of specialization, hopefully at high ranking universities. All aspects of this situation are under assault. Instead of passively screening an abundance of applicants who somehow showed up at the door, professors are recruiting. It is increasingly up to the student to find his or her own support, while the cost of graduate education moves ever upward. Hence, more and more students are already employed while in graduate school—many in nonacademic jobs—and more of them will be studying part-time. Professors will find it increasingly difficult to place all or most students, especially at high ranking institutions or in jobs that use the student's particular research focus. What we now consider as unusual and distressful was commonplace in the pre-World War II period, and the assumption by the professor and the institution of the responsibility for overcoming almost all student career risks is a relatively recent phenomenon. This recently acquired definition of professorial responsibility has introduced an immense load of the kind of guilt Kay expresses. I suspect the guilt will evaporate long before the situation changes for the better.

Second, our discipline is prone to a peculiar form of invidious comparison, which classifies sociologists' activities as belonging to either the core or the periphery of the field. Core is good; periphery is bad. The core is mainly populated by theoretical or methodological concerns. Substantively focused research and teaching are in the periphery. Sociological specializations that are both substantively defined and applied, such as criminology or demography, are automatically not in the core and are by extension of lesser quality and importance.

The same principle applies with double vigor to supplemental skills that lie outside the traditional confines of the discipline. For instance, a multidisciplinary language and area competence added to a general sociology training has the same effect as becoming a mother-in-law: it immediately lowers a student's perceived I.Q. by ten to twenty points. The bias against extradisciplinary supplemental skills, interestingly enough, is not shared by students. Language and area trained students, the group I know best, have already added one supplemental skill to their disciplinary training; many are now adding a third competence—a law degree, an M.D., a Master of Business Administration, a degree in City Planning or the Design of the Environment, to cite four cases on my desk right now. The students see this as an employment hedge, but the disciplinary department often sees it as a withdrawal from the field. A

brief telephone check around the country indicates that the language and area trained sociologists are doing comparatively better in the current market, although they too are seriously underemployed. It is still a little early to see how well those who are adding the third skill will do.

The division of the field into core versus periphery explains Kay's curious assumption that students trained for nonacademic employment will be unfit for employment in the academic market; such training, presumably, takes them out of the core. My feeling—my hope—is that the entire core/periphery formulation will collapse under the weight of current market forces; that concentration on a particular substantive area, even an applied area, will become reputable; and that the addition of supplemental skills will be seen as enriching, not corrupting, the field.

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I shall comment briefly on Paul Kay's three points in the light of my experience and understanding of how they apply or do not apply to sociology.

I see little evidence of a "burgeoning enthusiasm for nonacademic employment" (p. 217) among sociologists. Nor do I see any substantial evidence that the scholarly emphasis of most PhD programs in sociology is giving way to "practical" training and applied programs. To be sure, a number of sociologists have counseled such a shift—and many of their arguments have considerable appeal—but their ranks are relatively small. I come to this conclusion on the basis of correspondence with both academic and nonacademic sociologists, together with systematic participation in sessions on the topic, which have, over the recent past, been regularly sponsored by the ASA and many regional societies.

With respect to the likelihood of any massive proportionate increase in nonacademic job opportunities for sociologists, I come to pretty much the same point that Kay makes for anthropologists. The estimates with which I am familiar suggest that possibly one out of every

five sociologists is employed in a nonacademic setting, and that this ratio has not varied much over the past quarter century. This is not to say, however, that the *number* of nonacademic positions for sociologists will not continue to increase as it has for the past twenty-odd years. I am saying that there is no evidence to suggest that the ratio of nonacademic to academic sociologists is increasing either substantially or consistently.

I must take sharp exception to Kay's third point, that acceptance of the notion of increasing nonacademic opportunities simply serves to maintain the status quo. As I read the record of the past, the knowledge and skills of academic and nonacademic sociologists alike have turned out to be remarkably useful in constructive problem solving in an increasingly differentiated society. If the nonacademic base has not been proportionally broadened, its diversity in both public and private sectors most certainly has. This has been particularly true in positions calling for research. I list here but a few examples:

- most significant organizations that deal with inequality employ sociological experts, i.e., those dealing with minority groups, the poor, the handicapped, the exploited, the weak;
- most major religious faiths call upon sociological research;
- sociologists are playing roles in many aspects of the rapidly changing system of health care;
- sociological research models are frequently the key to problems posed by social program evaluation research (SPER);
- sociologists continue to work in areas of social planning such as community development, urban design, new towns, recreational facilities, market research, and sample surveys of many kinds;
- many major foundations employ sociologists in a variety of roles—of which program research is an important recent example;
- sociologists are typically found in what has come to be called policy research, i.e., activities centering around such problems as the future, population, the environment, social and biomedical ethics, international development;
- and, perhaps most recently, new research roles for sociologists are being developed in the business world as social accountability has taken on a new meaning in such areas as affirmative action, environmental regulations, consumer affairs, and corporate social responsibility programs in general.

And if the diversity of research roles being played by nonacademic sociologists is broad, their representation in administrative and executive positions seems to have kept pace: education (all levels), criminal justice, public administration, personnel, social welfare, human resources, labor unions, political parties, career counseling, social services—to list just a few.

My explanation for this record of diverse sociological participation in the "real" world is simple. It lies in the capacity of the sociological perspective to unlock many of the complexities and perplexities of modern life. What we take for granted and common-place in the meetings where we sociologists talk to one another, turns out to be anything but common-place in the world of affairs. Nonacademic sociologists have a lot to offer and there are many takers. While it may once have been true that nonacademic sociologists suffered from status anxiety and inferiority complexes, recent experience indicates that such feelings are rare in today's world. There are problems, of course, but they are problems which challenge the discipline, not its members.

My reaction to Paul Kay's argument as applied to sociology comes to this: sociologists should continue to be aggressively interested in developing nonacademic job opportunities, but I find little support for any notion which seeks to transform sociology into a practicing profession. Indeed, if I believed that this were a real possibility and that Kay's "ideology" had any viability for sociology, I would be inclined to ring the alarm. But I don't believe that we need to consider pressing the button just yet.

And finally, I disagree with Kay that nonacademic social science (whether anthropology or sociology) serves to maintain the status quo. Rather, it has been my observation that the aggregated outcome and consequence is just the reverse. As public expectations for the quality of life move higher and confidence ratings in social institutions move lower, sociologists are playing constructive roles both as researchers and in key administrative capacities. The sociological perspective demonstrates its worth in countless ways. It works, and it requires no tinkering at this juncture.

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My first reaction to the editor's invitation to participate in this exchange was to attempt to dissuade him from publishing it at all. I was not successful. I believe that Kay's arguments are both ill-founded and potentially damaging, both to my own discipline and its current and prospective practitioners, and to social science and scientists more generally. I feel strongly about the issues involved; while I see them from the perspective of my own discipline, I think that both his remarks and my comments

have implications for sociology as well as for anthropology.

I received traditional training in a large, prestigious anthropology department, and have more than twenty years of undergraduate and graduate teaching experience in rather conventional settings. I currently hold an exchange appointment for a year with the United States Environmental Protection Agency. Since 1973 I have been involved with committees, meetings and programs related to nonacademic employment.

No serious attention is given here to Kay's use of a Marxist frame for his essay. I think it contributes neither to Kay's argument, nor to my concerns about nonacademic employment.

Though he does not really say it forthrightly, Kay seems to argue that anthropologists have little to contribute in nonacademic work roles, referring to their "... incidental training in law, medicine, policy making, social welfare, public health and so on ..." (p. 217), which he concludes will prevent them from competing successfully in the labor market. By contrast, let me assert that there are at least three kinds of contributions which anthropologists can make in nonacademic roles in both public and private sector employment. All of them derive from good sound traditionally grounded training in the discipline. First, anthropologists are likely to contribute valuable perspectives from an appreciation of cultural relativism, especially important in policy building and planning for human affairs both domestically and abroad. Second, sensitivity to the "holistic" nature of cultural and social systems can usefully balance the rather narrower views of some other specialists with whom anthropologists ideally should work in complementary roles. Finally, anthropological use of and belief in the validity and importance of qualitative research methods and findings can support and improve the efforts of their work-peers in developing pertinent questions for survey research and the building of policy.

I am reinforced in my view of the importance of these items, having just finished reading and evaluating seven proposals by consulting firms for a large piece of social science contract research to be funded by a federal agency. In two of the research proposals, anthropologists are included as integral, central members of the research team. In a third proposal, no anthropologist is included, but extended and explicit reference is made to the importance in the proposed research of anthropologically-modeled qualitative research techniques as a prelude to the development of a major survey research instrument. Anthropologists *are* being recognized as important, and this recognition will increase if we do our work well.

Doing our work well includes learning how

to write plain English, discovering ways to frame problems and answers in terms that speak to the needs of the client (whether Congressman, bureaucrat or industrial executive), and committing ourselves to taking the risk of making decisions that affect human lives and resources, often within very limiting time frames. Added training in these areas will assist anthropologists to compete successfully in nonacademic labor markets. The need for these skills is shared, of course, with other social scientists who have long since entered the nonacademic marketplace in significant numbers.

Formal and informal surveys suggest that at least 600, and perhaps even more than 1,000 anthropologists with MA or PhD degrees already identify themselves as working outside the academy. Since academic anthropologists have tended to give such individuals short shrift until recently, these figures may well understate the real total. We know virtually nothing about holders of BA degrees in anthropology. Hence, nonacademic anthropologists already form a significant group when compared with the 10,000-odd membership of the American Anthropological Association.

Kay seems to think it unethical for anthropologists to compete for positions now held by others (he also rejects the notion that *net new* employment is possible). I find this thinking both unreasonable and unanthropological. As social and economic systems evolve through time, the need for various sorts of professionals also changes. Present jobholders' skills may be needed less in the future, or in other roles. As these shifts occur, anthropologists offering useful skills will be met with greater familiarity and receptiveness by employers (we still suffer widely from the myth that anthropologists restrict their attention to "stones and bones"). In any case, I see anthropologists as complementing, not simply replacing, other workers. It is in the application of various analytical skills that optimum benefits to the society will result.

Extending his argument against training for nonacademic employment, Kay asserts not only that nonacademic anthropologists will find new jobs available, but that they will maladapt themselves yet further by taking themselves out of the academic marketplace. I think this argument badly misunderstands training needs for prospective nonacademic anthropologists. They need not *less* traditional training in anthropological theory and lore, but *more* training in skills adaptive outside the academy (see above). Hence, contrary to Kay, I think that well-trained nonacademic job candidates will be more broadly adaptive than their colleagues with solely academic orientations.

Kay appears to me almost more interested in argument than in data. While some proponents of nonacademic training to seek additional employment may be guilty of excesses, others of us would take serious umbrage at Kay's implication that our motives in promoting nonacademic pursuits are simply self-seeking and self-serving. I believe, as Sol Tax has argued for decades, that anthropology has important contributions to make to the welfare of humans generally.

To cite Angrosino et al. (1977:18), to whom this brief essay has an obvious and important debt,

... (Kay) tells us that nonacademic anthropology is nothing more than an academic conspiracy to maintain the status quo. Then he looks at the professions which have managed to gain wide recognition outside of academic settings, and tells us that we ought to stand clear and let them be. Importantly, Kay fails to consider the possibility that anthropology, based on the varieties of "anthropological perspective," might have something special to contribute to the rest of the world.

Ultimately, is it not contributions to the general welfare which serve as the essential defense for the existence of and support for any intellectual discipline?

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REJOINDER

I would like to thank *The American Sociologist* for reprinting my little essay and the nine people who took the trouble to reply to it. Since I am informed by the Editor that these final comments should occupy about the same space as each of the nine replies, I will not be able to address the replies individually but will have to respond to the major themes they express.

On the whole, although with some demurrers such as Gollin and Hurwicz, there seems to be agreement among the commenters (a) that social science generally is facing an oversupply of PhDs and (b) that there is substantial adherence in some quarters to the claim that the glut can be alleviated by expanding nonacademic employment. It is understandable that Gollin and Hurwicz find my expression "burgeoning enthusiasm" an overstatement: Gollin, I note, is Chairman of the ASA Committee on Expanding Employment Opportunities, and Hurwicz cites two publications of her own dealing with the topic of nonacademic employment. These two academics are apparently devoting a substantial portion of their professional time to the subject of nonacademic employment. While one can sympathize with the complaint of any specialist that his or her specialty is not receiving the attention it deserves, the very emergence of nonacademic employment as an area in which academics can publish papers and chair committees would seem to support my point.

Similarly, although some commenters express doubt regarding the degree to which what I describe for anthropology applies to sociology, most state or assume that the current employment situations of the two disciplines are essentially the same. That is, the consensus of the comments is that my argument is either right for both disciplines or wrong for both disciplines, but probably not right for one and wrong for the other.

Several commenters are worried by the lack of statistical support. (Sibley, while taking me to task for being "more interested in argument than in data," [p. 231] furnishes as the sole authority for the numbers he gives anonymous "formal and informal surveys" [p. 231].) My piece presented no statistics. Reliable data in this area are hard to come by, especially on the demand side. On the supply side, however, there are some hard numbers, for anthropology at least. According to the listings in the *Guide to Departments of Anthropology* published by the American Anthropological Association over the past ten years, 1968–1978, there has been an almost perfect linear increase in the number of new PhDs produced per year; Pearson $r = .96$; mean annual increase in PhD production = 35. There are not going to be enough jobs in the academy for these people; in fact there already aren't. Now, one perfectly reasonable attitude to take is Professor Borgatta's: Tough! A Hobbesian morality such as Borgatta's is as internally consistent as any other. From such a moral outlook, the concern felt by some academics that they are systematically inducing aspirations in PhD students that are doomed to bitter disappointment is just more of the same old humanitarian eye-wash:

"a no-failure ethos . . . that no person should have to suffer the indignities of not getting all to which he/she aspires" (p. 222). The virtue of Borgatta's position is its honesty. Borgatta's ethos is "Let them eat cake"; he stands squarely with Marie Antoinette in the forthright belief that the "pyramid" is divinely ordained and needs a substantial base, occupied by those who fail to rise.

It is not toward attitudes like Borgatta's that my remarks were addressed. My point was and is that academics, most of whom fancy themselves liberal, progressive or humanitarian, are deluding themselves—note, *not* engaged in a conscious conspiracy as some of the commenters have incorrectly inferred—that somehow the nonacademic sector is going to absorb the PhDs they continue to produce at a constantly increasing rate. This is a convenient fiction because it jibes with their liberal consciences.

Look at the matter this way. We have a supply and demand problem: too many PhDs for the academic market. There are three ways to approach such a problem. (1) Ignore it à la Borgatta; the invisible hand of impersonal market forces, if not trammled by misguided do-gooders, will assure that all resources, including people, get allocated in the way that provides the greatest good to the greatest number. (2) Increase the demand. (3) Reduce the supply. The first, the laissez-faire policy, does not square with the humanitarian consciences of most academics because they can't stand to think of the PhDs they have personally trained living ruined lives—even if this suffering is "necessary" or even socially beneficial from a certain (intellectually legitimate) point of view. This reduces the alternatives for these individuals to (2) increase the demand and (3) reduce the supply. In anthropology, as I pointed out, there has so far been a great fuss made about implementing (2) but not a public peep about trying (3). Is this rational? I submit not. Certainly academics have a great deal more control over the supply side of the PhD employment equation than they do over the demand side. Why not try to affect the side of the problem over which we have the most control? Answer: because reducing the supply, cutting graduate enrollments, would mean seriously inconveniencing ourselves.

My claim was that the current brouhaha about expanding nonacademic employment for PhDs is motivated more by the maintenance of the current level of convenience for existing academics than by any real evidence that there is or will be a sufficiently large nonacademic market to dissolve the PhD glut. Nothing in the responses to that claim shakes this main argument. What then should we do? By all means continue every real effort to expand nonacademic employment opportunities but

also expend equal or greater effort to reduce graduate enrollments. In one sentence, Blalock goes to the heart of the matter: "We may be heading for disaster down the road unless we

begin to restrict the number of new PhDs turned out each year" (p. 219).

Paul Kay

THE NEW ACADEMIC HUSTLE: MARKETING A PhD*

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When we received our PhDs in 1976, we knew the academic labor market was tight. We knew we would have to look hard for the kinds of jobs we wanted. But we did not anticipate the often discourteous, unfeeling and degrading reception we were to encounter as job applicants. In three years of job-hunting, we accumulated over 200 rejection letters, had 21 convention interviews between us, and visited many campuses and offices for on-site interviews. This experience convinced us that, at a minimum, a code of ethics is needed to govern the behavior of recruiters and candidates in the employment search. In addition, academic departments, professional associations and job candidates themselves must search more vigorously for other job markets and for possible solutions to the current employment crunch felt by new PhDs.

"... the shortage of Ph.D.'s constitutes our most critical national problem."

John F. Kennedy¹

In 1972 George McGovern was soundly defeated in his bid for the presidency, Floyd Patterson was KO'd in his attempt to regain the heavyweight boxing championship of the world, and we entered graduate school in sociology at Michigan State University. Unlike McGovern and Patterson we had modest goals. We were not trying to rise to the top of our profession; all we wanted was to break into it. We completed our graduate studies early in 1976, and by the end of that year had completed over two years of unsuccessful job hunting. Like McGovern and Patterson we fell flat on our faces, but with PhD in hand. As it turned out, we were not alone.

Between the time we began our graduate work and the time we finished, the job market for sociology PhDs simply nosedived. In 1970 there were approximately 900 new academic slots for 585 PhDs in sociology (Demerath, 1971:87).

But by 1975, 501 new PhDs were competing for approximately 250 jobs (Wagenaar and Newby, 1971:1). Even allowing for the fact that 25% of sociology PhDs generally find suitable positions, the PhD job market has clearly taken a downturn. And things are worse in other disciplines. Out of 1,225 history graduates and doctoral candidates who sought academic jobs in 1973, only 182 were successful (Freeman, 1976:28). Freeman concludes that the 1970s have produced "the worst job market for Ph.D.'s in American academic history" (1976:91). The frustration reported in the odyssey of Anonymous (1976), the poetry of Ledger and Roth (1977) and informal discussions in graduate departments across the country affirm Clark Kerr's assessment that:

... it is tragic to encounter new Ph.D.'s who started working for their degrees when their subject fields were booming only to finish them when the market for their training had all but disappeared. (Cited in Cartter, 1976:forward)

Between November 1974 and July 1977, we sent applications to, and received rejections from, over 300 potential employers including academic departments, public and private research organizations, human service agencies, advocacy

* This is a revised version of a paper titled: "Buddy can you spare a job: Rejection in the halls of academe," presented at the 1977 Meetings of the Society for the Study of Social Problems, Chicago. [Address all communications to: Gregory D. Squires, U.S. Commission on Civil Rights, Room 3280, 230 S. Dearborn, Chicago, IL 60604.]

¹ Cited in Freeman, 1976:2.

groups, government agencies, newspapers, magazines, public opinion polling services, foundations, and a variety of private industrial concerns. What follows are our reflections on the job hunt experience.

Not all new PhDs shared our experiences, of course. One friend of ours, who recently completed his doctorate in rural sociology, sent out only a handful of applications. He received three offers, and is presently in a tenure line position at a prestigious eastern university. A few newly minted sociologists, because of their skills, areas of expertise, ol' boy connections, or some combination of these factors, do find themselves in the enviable position of having to choose among several attractive job offers. We were not so lucky.

FIFTY WAYS TO LEAVE YOU CRYING

Rereading three years worth of rejection letters is, much to our surprise, quite an amusing experience. The letters came in all shapes and sizes from personally typed and signed statements to badly scrawled, unsigned post cards. (Our discussion of letters of rejection is based on the 240 we received from academic employers.) The shortest letter received was barely two lines long, and only a slight fraction of the letters reached 15 lines in length. Close to 90% "thanked" us for the interest we showed in their departments, over half "apologized" for the bad news contained in the letter, and about 40% wished us "good luck" with our future careers.

Approximately 10% of our rejections were simply that, straightforward rejections. One example read: "This is to inform you that the assistant professorship position in our department for which you had applied has been filled." About half the letters were written in the same straightforward tone, but simply added a few more words to get the job done. An example read: "We regret to inform you that after carefully reviewing your vita and materials available to us, the faculty search committee has determined that your qualifications do not appear to match our departmental needs sufficiently to warrant further consideration of your

candidacy." Over 16% of the rejection letters commended us for our high qualifications and abilities, but with the same result: "Your credentials are excellent. The interests and experiences of a few other candidates matched our needs somewhat more closely—hence we made our selection not on the basis of capabilities only, but in terms of a match with the position available."

Occasionally, however, we received letters in which the author expressed genuine concern for the plight of today's job hunting PhDs and offered a brief "pep talk" in the rejection. Such letters kept our spirits buoyed and our vitas circulating.

A THOUSAND CLOWNS: INTERVIEWING AT THE ANNUAL CONVENTION

Many soon-to-be PhDs supplement their letter writing campaigns by registering with the employment services at regional and national conventions. Between the two of us, we had 21 interviews at the 1975 and 1976 ASA conventions. While most of our convention interviews came via the employment service, others came about more informally. Despite the growing concern with affirmative action and more open, formalized employment practices, we found the ol' boy network is still a most effective mechanism for securing an interview, even though it can also lead to some awkward moments. One of us was chatting with an ol' boy from Michigan State in the main lobby of the hotel at the 1975 convention when an ol' boy from another school was checking in. Tom relates the ensuing sequence of events:

I was introduced to Art and it was suggested by our mutual friend that we go up to Art's room for a little talk. Art looked me over and glanced down at his suitcase. Being sensitive to nonverbal cues I picked up his bag and headed for the elevator. While unpacking, Art asked me a few questions. Unfortunately nothing came of this encounter. Apparently I came across as a better bellhop than sociologist.

Whether our convention interviews came via the ol' boy network or through the employment service, we confronted four basic types of interview situations.

Boot Camp

During some interviews the recruiters take on the aura of drill sergeants as they attempt to wreak as much verbal carnage on the candidates as possible. The lowly graduate students (and they are assured of their lowly status) are subjected to insults about their training, tirades against their major professors' work and unfavorable innuendoes about the quality of their departments. The best is left, however, for the dissertation. Candidates are frequently informed that their research is outdated and will make no contribution to the field. Further, recruiters may emphasize the prestige of their own departments, and stress that junior faculty members are expected to publish often, and in the most prestigious journals. Presumably, those candidates most capable of digging themselves out of the ground will be invited on campus for a further drubbing.

The Fraternity Rush

At the other end of the spectrum are those interviews where the most rigorous question is, "Hi, how are you?" These discussions consist of little more than the rah-rah most college freshmen receive during fraternity rush week. The "brothers" (and occasionally "sisters") give a brief description of the school, the community where it is located and how the sociology department is developing, and assurances that the candidate might well fit into the department's future plans. While previous experience, current interests, and potential for professional accomplishments must be a factor, and such information can be at least partially gleaned from the vita, no attempt is made in these interviews to obtain further substantive information about the candidates' professional qualifications.

Methodus Ignoramus

In the most frustrating type of interview, recruiters focus solely on the candidates' technical-methodological competencies. Virtually all of the questions during these sessions deal with statistical or analytical approaches currently in

vogue in the discipline. Candidates are asked to describe sampling procedures, their sources of data, statistical techniques, assumptions underlying such techniques, why other types of analysis or measurement were rejected, etc. These interviewers brush off as irrelevant and inconsequential the more meaningful theoretical issues under investigation in the candidates' dissertations.

Apparently, technicians rather than intellectuals are being recruited. A recruiter from one Ivy League school said the reason for this emphasis is that new faculty members have important teaching responsibilities. In other words, it is not enough that the current faculty consist of technicians, but it must be able to turn future generations of budding young scholars into technicians as well.

The Exhibition Game

Frequently recruiters appear to be just going through the motions. In these discussions the interviewers do little more than ask a set of routine questions about the candidate's teaching and research interests and dissertation topic, pausing only briefly between questions for the individual's response. The general impression offered by the interviewers is that they just want to get on to the next interview. Perhaps these recruiters are just practicing, or perhaps they are waiting for those candidates in whom they have special interest, but more likely an entire day of one 15-minute discussion after another holds no appeal for them at all. Though these recruiters are pleasant enough, the interviews just do not seem worth the time to either party.

HALFWAY HOME: THE ON-SITE VISIT

The immediate objective of the convention interview, of course, is to coax, cajole, beg, or even earn an on-campus or on-site interview. Between us we reached this plateau ten times. These exercises ranged from two-hour to two-day interviews on college campuses and in federal and state office buildings. What follows is a synopsis of four interviews we have endured. First Tom describes two interviews

he had for academic positions and then Greg reviews two interviews he had for government jobs. Again, we make no claim about the representativeness of these events, but they do indicate what others coming off the PhD assembly line might encounter when they place their wares on the market.

Shot Down in the West

After two years of an unsuccessful job search, I was extremely pleased to learn that I was one of four candidates to be interviewed for a teaching position at a western state university. I hit it off with most of the faculty members: our discussions were stimulating, the \$10-a-plate dinner we had was quite tasty, and I discovered that we shared many nonprofessional interests. When the chairman dropped me off at the airport, he told me the faculty enjoyed my visit and I would be hearing from him soon. The opportunities looked promising and I anxiously awaited his phone call. What I received instead was a two sentence, unsigned letter of rejection.

Southern Hospitality

I had a most unusual experience in the fall of 1976 at a small southwestern college. Both evenings of my two-day interview were spent barhopping, visiting seven different taverns in all. Despite the number of watering holes, I had reservations about my social life in this relatively isolated hamlet. I half-jokingly mentioned to the chairman of that department that, being single at the time, I did not want to take a position in a town with such an apparently limited social scene. I nearly fell off my chair when he quipped, "You want girls, we can get you girls."

Since I still had what I thought were attractive job possibilities outstanding, I politely declined when I was offered the job a few weeks later.

The HEW Crusaders

While being interviewed for a position with HEW involving research on educational inequality, I was asked by one staff member, "What do you think of civil

rights?" I could tell this was a sensitive issue for this particular white male so I responded as ambiguously as possible. When I asked what he thought of civil rights, he informed me that he had had his own business until he went broke trying to meet all the federal regulations. His voice turned particularly bitter when he referred to "all the damn quotas" he was forced to meet.

Next he asked me if I was a crusader and stated, "We have no room for crusaders here. We have a job to do and we cannot let personal interests interfere with doing that job." The discouraging implication of this discussion was that an active interest in civil rights made one a crusader and, therefore, unqualified for a job involving research on inequality in education. Although this interview took place in the spring of 1976, I have yet to hear officially whether or not I got the job.

The Great State of Michigan

One of Michigan's human service agencies invited me down to the state capitol to be interviewed for a program analyst position in the fall of 1976. The director of the division began the conversation with, "This job pays only \$15,000. If I had a PhD I wouldn't work for \$15,000, why are you willing to?" Not wanting to show how desperate I was for a job, I mumbled something about how the nature of the work is more important than the pay. He proceeded to describe the job. "You will take strict orders from me. Things can get hectic and there will be a lot of pressure. I will rely on you take some of it off me. You're my assistant, you will be like a secretary to me."

Three days later I received a call from the personnel office. They wanted first to "clarify" the salary. The \$15,000, it turned out, included a host of fringe benefits, but the salary itself was only \$12,500. Then I was offered the job, which I politely turned down.

We do not want to discourage job seekers from accepting invitations for interviews. After all, it is a necessary step in getting a job. We simply want to warn them that they should be prepared for some unorthodox experiences.

WHAT CAN BE DONE

The Kinds of abuses we (and others) endured serve to compound the trying circumstances recent recruits into the sociological fraternity are facing. While such personal indignities cannot be totally eliminated from our world, steps can be taken to minimize them for job seeking PhDs. We recommend, as a starter, that the ASA Committee on Expanding Employment Opportunity develop a code of ethics governing the behavior of recruiters and candidates in the job (or candidate) search. That committee should advertise the fact that it is developing such a set of guidelines and encourage job seekers and recruiters to submit documentation of troubling experiences they encounter.

Among the kinds of expected behavior that should be recommended in the code would be the following. Employing institutions should promptly notify candidates about the status of their applications when candidates respond, in writing, to advertisements announcing job openings. The potential employer should inform candidates how many openings are anticipated, how many applications have been received, when decisions will be made regarding who will be invited for an on-site interview and who will be selected for the position, and what, if any, action has been taken on their applications. At conventions and elsewhere institutions should interview only those candidates they are genuinely interested in learning more about in terms of their professional background and expectations. In turn, candidates should only interview with institutions they genuinely want to learn more about. In other words, there is no room for the "exhibition games" that frequently take place. Following all interviews, institutions should promptly notify candidates what action has been taken on their applications, and when a final decision will be made. At the same time, candidates should promptly notify institutions whether or not they will accept an invitation for an interview or for a position if such offers are extended. When a negative decision is made at any stage of the job search (by either the institution or the candidate) a specific reason for that decision should be provided in writing. Simply

informing a candidate that "the assistant professorship position in our department for which you had applied has been filled," is not sufficient. Potential employers and employees should treat each other in the professional way they would want to be treated if they were on the other side of the table. The humiliation of an army boot camp and the artificiality of a fraternity rush have no place in a job interview. These are examples of the kinds of guidelines that should be included in a code of ethics; they are not meant to be all-inclusive.

Once the code is developed, it should be widely distributed among potential employers of sociology PhDs. Obviously, this step will not seriously dent the ol' boy networks, which still exercise considerable influence at all stages of the job hunt. A voluntary code cannot eliminate these abuses. But it would enhance general awareness of the problems and would have a favorable impact.

These abusive practices would be drastically reduced, of course, if the larger problem of the job crunch itself could be resolved. Although some action has been taken toward this end, there is more that academic departments, professional associations, and job candidates themselves can and should do.

For example, faculty members at PhD-granting institutions should try harder to seek out more nonacademic employment opportunities, and to assist, or at least not discourage, students in the pursuit of such jobs. Unfortunately, academic employment is often regarded with more favor than other kinds of work, and students are frequently encouraged, not so subtly, to forget about "lesser" pursuits like government service or private sector positions. Informal networks that have been so effective in placing candidates within academia could and should be extended to both governmental and private sector employers. One might argue that ideally such networks would not operate at all in the job search. But they exist and are likely to continue to operate in the near future. If they are expanded, as recommended here, job searchers as a group only stand to benefit.

For new PhDs who for whatever reasons are unable to secure suitable work

and who find their financial support terminated upon completion of the dissertation, we recommend that graduate departments, the ASA and others act to establish more post-doctoral teaching and research opportunities. One example is the series of special grants created by the Society for the Study of Social Problems for unemployed and underemployed sociologists to conduct research on the PhD job market. Cities, churches, and a host of other organizations currently receive CETA funds for temporary employees. Academic departments should more actively seek such financial support. Graduates often take part-time teaching positions at local junior colleges while pursuing permanent positions. PhD-granting institutions should establish more formalized relationships with other educational institutions in their communities to expand such opportunities. These steps would not completely resolve the job plight facing new PhDs, but they would ease the crunch, at least temporarily during these particularly tight years.

Candidates must also initiate more vigorous job searches. In addition to following the advertisements and interviewing at national, regional, and statewide conventions, they should plug into existing ol' boy networks whenever possible and they should also attempt to establish their own connections with potential academic and nonacademic employers. One effect of such tactics, of course, would be simply to heighten the already fierce competition for existing jobs. However, if graduates as a group more actively and creatively seek out employment possibilities, at least a few perhaps unconventional opportunities will open up. An example from philosophy is instructive. After finding a philosophy PhD to be a competent law enforcement officer, the sheriff of Golden, Colorado took out an advertisement in the American Philosophical Association bulletin, *Jobs in Philosophy*, which read in part:

Usual law enforcement duties...Ph.D.'s with specialization in classical and/or early modern philosophy preferred, with well-developed competencies in the history of philosophy...All basic equipment furnished except weapons and boots. (Harrison, 1976:64)

The proverbial cab-driving PhD is not yet commonplace, but the job crunch is quite real. It is unfortunate that the plight of today's job-seeking sociologist is often compounded by unprofessional, insensitive, and occasionally exploitative behavior on the part of some of the discipline's gatekeepers. We hope that appropriate action will be taken on several fronts so that prolonged job searches such as ours will become a quirk of the 1970s, rather than additional standard dues that new initiates will have to pay.

POST SCRIPT

By September 1977 both authors were fortunate enough to have found reasonably secure employment in their fields. After working for six months as a temporary assistant instructor at Michigan State University, Greg accepted a position as a researcher for the U.S. Commission on Civil Rights in Chicago. Tom worked for six months as a manpower analyst for the Michigan Department of Labor and six months as a temporary research associate at Michigan State University before accepting an assistant professorship in the Department of Agricultural Economics and Rural Sociology at Clemson University.

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TOWARD AMATEUR SOCIOLOGY: A PROPOSAL FOR THE PROFESSION*

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Other sciences, among them history, archaeology, mineralogy, ornithology, astronomy, and entomology, have profited greatly from their vigorous amateur wings, which have existed along side of and sometimes preexisted the now dominant profession. The greatest single contribution of amateur science societies to their associated professions is the collection and dissemination of descriptive data. Intensive research on two of these amateur sciences and examination of the descriptive-historical literature of all six suggest that we ought to think seriously about promoting an amateur sociology. After a description of avocational science in general, a proposal for an amateur sociology is presented, using the avocational developments in the other sciences as a model.

Sociologists, perhaps blinkered by a self-conscious professionalism, have largely ignored the serious, highly effective traditions of amateurism that have existed for many years in several other sciences. Around the world amateurs are conducting projects and gathering data that are valuable contributions to their particular disciplines. Amateurs were chiefly responsible for observations of the lunar occultations that helped establish more accurately the location of the moon, thereby making it possible to land on it. Amateur ornithologists, especially women, conduct much of the leg-banding research in bird migration. In entomology amateurs do most of the mapping and enumerating of insect populations in various localities. In several fields the amateurs preceded the professionals, establishing and developing those fields to the point where full-time work could be carried out in them.

David Riesman (1954) must have had such contributions in mind when he called for the development of an amateur sociology in a short section of *Individualism Reconsidered*. He briefly described the British group known then as Mass Obser-

vation (MO), an essentially adult education organization in which amateur social observers were encouraged to forward reports on assigned topics to a central office run by social scientists.¹ He thought the idea sound enough to be worth importing:

At any rate, America much more than England seems to me to need both the reporting technique and the enlistment of the amateur observer that characterize MO—to need them for the basic job of finding out what goes on. . . . This country is so big, so varied, so almost if not quite encompassable, that social research cannot have enough observers who will break down its momentary generalizations and open up new views. . . . By bringing these observers into our research organizations, moreover, we are likely greatly to amplify our conception both of the complexity of our country and of its newly emerging problems, for there will be no want of stimulating queries and reports. (Riesman, 1954:481-482)

Riesman's appeal stirred little visible interest, perhaps because many professional sociologists were apprehensive about the conduct of an amateur sociology, a sentiment Riesman failed to consider. Some colleagues have argued, for example, that the field is too recondite, with its high-order abstractions, methodological debates, ambiguous terms, and countless neologisms, to be mastered by amateurs. They also point out that the collection of data, a delicate matter at the best of times, is no job for an amateur. The thousands of pages of discussion on

* This paper was presented as part of the Maurice Manel Lectureship, Atkinson College, York University, 23 February 1978. I wish to thank Merlin Brinkerhoff, Kathy Burke and William Zwerman for their useful comments. This proposal, in part or in whole, does not always represent their views on the establishment of an amateur sociology. [Address all communications to: Robert A. Stebbins, Dept. of Sociology, Univ. of Calgary, 2920 24 AVE., NW, Calgary, Canada T2N 1N4.]

¹ As near as I can tell this organization no longer exists, and nothing similar has emerged in its place.

how to gather, interpret, organize, and publish them attest this point. Moreover, why should we want to promote amateurism when we are still in the process of professionalizing?

Nevertheless, my research convinces me that these sorts of misgivings are founded on misconceptions of how avocational scientists actually function in their leisure calling. I will attempt to dispel many of these misconceptions in the first section of this proposal by describing the nature of amateurism and avocational science. I base my discussion on an intensive study of two amateur sciences, archaeology and astronomy, and the descriptive-historical literature on amateurs in these two fields and four others: archaeology (Tivy, 1976; McGimsey, 1972; Hole and Heizer, 1969:53-54; issues of state archaeology society newsletters, bulletins, and journals); astronomy (Dodson, 1964:190-191; Krogdahl, 1952:561-564; Sidgwick, 1958:14-16; issues of *Sky and Telescope*); ornithology (Mayr, 1975; Maltais, 1978:20-22); mineralogy (Desautels, 1969:220-233); entomology (Borror and DeLong, 1964:737; Watson, 1975; *Entomological News*); and history (Finberg and Skipp, 1967:44, 70; Thompson, 1967:333-344; Higham, 1965).²

AVOCATIONAL SCIENCE

At least four conditions encourage the emergence and persistence of an avocational interest in a science: (1) There are believed to be many undiscovered phenomena of scientific significance. (2) Many important characteristics and behaviors of known phenomena have yet to be systematically studied. (3) These phenomena, characteristics, and behaviors are observable in their natural environment with the naked eye or with relatively inexpensive specialized equipment. (4) The number of professionals is too small to complete this work alone.

Avocational science, however, is but one important development in the larger twentieth century movement of modern

adult amateurism (Stebbins, 1977:582-584). Amateurism today exists in four professional areas: art, science, sports, and entertainment. *Modern* amateurs are serious participants not interested in play and dilettantism but in usefulness, obligation, and systematic commitment. They operate within a professional-amateur-public system of functionally interdependent relationships, where they are differentiated from their professional counterparts by a characteristic set of attitudes.

In my comparison of amateur astronomy with amateur archaeology (Stebbins, 1978), I draw a distinction between *observers* and *armchair* participants. We are chiefly concerned with the former in the present paper; they are active avocational students of a science who directly experience their objects of scientific inquiry. Observers fall into three subtypes: apprentices, journeymen and masters. The apprentices are fundamentally learners. The journeymen are knowledgeable, reliable practitioners who can work without supervision, albeit only within one or two specialties. That is, they have learned enough to be able to make an original contribution to their science. Only the masters, however, actually do this, for they not only collect original data, but also disseminate it publicly through talks, reports, journal articles, even monographs.

The dominant orientation in avocational science is active research, not armchair reading. But it is usually the observational research of exploration rather than the controlled research of experimentation. Amateur scientists tend to lose ground to the professionals as the problems to be dealt with grow more abstract. Some amateur geologists outdo their professional colleagues at on-the-spot recognition of minerals (Desautels, 1969:220), but I found that most amateur archaeologists needed and wanted supervision in classification of their materials. Hypothesis formation and theory construction are rarely done by the part-time scientist.

Contacts with professionals are cherished, for these scholars are both objects of a great deal of respect and models to be emulated.³ And amateur scientists

² Electronics has its ham-radio operators and political science its local amateur politicians (Wilson, 1962), but these are applied specialties. The aims and values of these participants differ considerably from those of the pure science amateurs.

³ It is my impression that pure amateurs who see the professionals at monthly meetings and possibly in

soon learn that nothing catches the attention of a professional like research, especially research that is somehow publicized. Consequently, the master amateur enjoys the most intimate association with professionals; this in itself is a strong incentive to develop knowledge and skills. The amateurs appear to be widely accepted in archaeology and astronomy (especially the latter), though there are professionals in these fields who decry their presence.

There are still other reasons for pursuing the serious leisure of avocational science. For the master, and even the journeyman to some degree, there is recognition associated with doing research and particularly with disseminating it. Other rewards include self-enrichment, self-actualization and self-expression (Stebbins, 1979). Though of secondary importance, the social and re-creative qualities of avocational science are also attractive.

Amateurism is far from being wholly pleasurable, which is why it is a sociological curiosity in today's fun-oriented world. The committed avocational scientists with whom I have had contact seem prepared to endure nearly endless physical hardship and frustration in the pursuit of research data. Archaeological excavation, for example, is arduous. Digging, hauling, and sifting dirt for hours on end in an environment of insects, sandburs, heat and humidity requires stamina and devotion (in a sample of Texas amateurs). Amateur astronomers may have to rise at three o'clock in the morning and drive sixty miles from the city (to escape its "light pollution") to view a particular phenomenon. Winter is the best observing season for Alberta amateurs, but nighttime temperatures may fall below minus 30 degrees Celsius.

Avocational scientists are usually organized into self-supporting local clubs or societies. Here, typically, the adult pure amateurs (among them those with BA or BSc degrees) far outnumber the adoles-

cents and preprofessional graduate students who, in turn, are moderately more numerous than the professionals.⁴ Depending on the science, in North America these groups may be federated into state, provincial, regional, even national bodies. The local units are the basis of it all, however, for the amateur has no institutional funding for distant conferences or research trips. He or she may venture in search of data as far as a day's drive from home, but scientific orientation is sure to be local. Curiosity about the nearby physical or social environment and its history is a common reason for joining a science society.

Member screening procedures vary from discipline to discipline. In archaeology they are more elaborate than in astronomy, perhaps because an improperly trained practitioner can do damage. The screening may include a personal interview with a society official, a review of the application by local and national councils, an examination of the applicant's experience in the science (research, reading, courses), and a consideration of the person's reasons for joining. Attendance at a minimum number of meetings may be required as well.

The standard purpose of these societies is to promote the sciences they represent. This is done at the monthly meetings through educational lectures or demonstrations and presentations of original data by members or invited speakers. Professionals figure prominently here. Small numbers of them can be found in most local societies, where they perform a yeoman service in advising on research, giving talks, and linking the amateurs to the professional side of the field. The local societies further promote their sciences through such public service functions as displays (e.g., in museums, libraries), lectures (e.g., to school, religious, youth, service groups), and special events (e.g., "star nights" in astronomy).

AMATEUR SOCIOLOGY

The purpose of an amateur sociology society would be to promote community

connection with research hold them in greater esteem than does the typical university major in the field. The latter commonly sees little of professionals where things count most for them; namely, when they are doing research or analyzing it. Rather the student must face them day in and day out in the classroom while their hearts lie elsewhere, a situation that tends to destroy mystique.

⁴ Further discussion of "pure," "preprofessional," and other types of amateurs is available in Stebbins, 1977:594-596.

interest and education in and original research contributions to the science of sociology. This could be done in much the same manner as just described—through internal and external lectures and data presentations. But sociology itself would be most advanced by the actual research of amateur members. To be of benefit, such research would have to be disseminated, preferably in reports or journal articles (dare I say books), but at least in talks before the society or before state, provincial, or regional professional conferences. Data, whether gathered by professionals or amateurs, are of no use if kept secret.

As in other fields, professional sociologists would advise on research, present talks, serve as a link to the profession, help arrange for guest speakers, and so on. They would likely be voted into certain offices from time to time and would certainly be a source of answers to innumerable substantive and methodological questions.

Data Collection

What kind of data could an amateur sociologist collect? As mentioned above, avocational scientists tend to confine themselves to the observational/exploratory level of science; that is, they count, measure, collect, or describe some phenomenon. The amateur sociologist would do the same. There are more unexplored spheres of social life and patterns of behavior than professional sociologists will ever get around to examining. A devoted auxiliary of avocational scientists would at least help us chip away at this Gargantuan task.

I am certainly not advocating that amateurs independently conduct their own interview projects, though they might interview under the watchful eye of a professional. This is one place where it is possible for them to do damage. But amateurs could observe systematically in public places where there is neither an entry problem nor a need to state one's purpose.⁵ What people do in public is for

public consumption. Amateurs could record the spatial positioning of people in waiting areas, describe the behavior of children in queues, observe the patterns of interaction at a bar or carnival, count the number of people entering and leaving various sorts of commercial establishments, take note of overheard conversations about an emergency, use city directories, content analyze magazine advertising, and so forth. The list is endless. While such independent observation may make some professional sociologists uneasy, the writings of professionals in other sciences and my own research indicate that experienced amateurs can contribute valid data without supervision.

Amateurs could also report on the goings-on in their own lives if they were willing (and they might be if anonymity could be assured for all concerned). They could describe relations at home, at the lodge or club, behind the boardroom door, at the health studio, among friends while hunting or bowling, around the service station, or in the political party caucus. Such reports might occasionally become available on parts of community life that we professionals find it difficult if not impossible to study. And we would certainly get divergent viewpoints, for amateurs in a particular art, science, or sport come from many walks of life (Stebbins, 1976).

With a proper understanding of scientific objectivity and generalization, amateurs could also comment on the fit of theory and the data that have accumulated on their occupational, leisure, familial, and other life roles. Is it not the negative case or the unexplained variance that is supposed to catch the scientist's fancy? Events that fail to fit propositions will help lead to theoretical improvement.

Amateurs would not necessarily have to design and carry out their own studies, although the initiative and independence that this requires would undoubtedly appeal to many as "avocational entrepreneurs" (see Stebbins, 1978). Recall that Riesman made his appeal in a book about individualism. Still, a professional could invite the assistance of a group of am-

⁵ As one reviewer of this paper suggested, there may be regional and national variation in the laws pertaining to what might be called social scientific

voyeurism. Amateurs as well as professionals who are interested in observational research would need to be aware of these laws.

ateurs for one or another project. Their reward would be the professional contact they treasure and, it is hoped, joint authorship or other appropriate acknowledgement of their contribution.⁶ Such assistance would be free, for monetary return is furthest from the minds of avocational scientists. To the contrary, they are quite prepared to spend money on their leisure, if they benefit from the participation made possible by this expenditure.

I hope it is clear that I am not proposing a popularization of sociology. Someone with more journalistic skills than amateurs or professionals normally possess is needed for the job. And would-be Vance Packards, if they came under the influence of the avocational sociology societies being discussed here, should develop scientific values that preclude slipshod data collection, unwarranted generalization, reliance on anecdotes, and unethical research practices. Journalists such as Jane Jacobs and Michael Harrington avoid these flaws in their work, which is why it is respected by many professional sociologists. Amateurs could be trained to make valid sociological observations by following certain key rules that would distinguish their efforts from those of journalists, who are less acquainted with scientific procedure.

Training

Abstract and complex research skills having already been identified as beyond the ken of most amateurs, the job of training them is simplified considerably. Certain key rules should be imparted, for example: (1) "All field researches are guided by the intention to transcend the subjective observational categories of the researcher—to base the observations in intersubjective categories, at least insofar as this is possible. . . ." (Johnson, 1975:21–22); (2) the observer should be constantly aware that people can, and often do, define differently any situation in which they happen to be mutually present; (3) the importance of accurate re-

porting should be stressed, as well as (4) the need for detailed description of the events observed; (5) the observer should realize that a single anecdote can never count as a generalization, but only as a suggestion of one. Where an amateur is attracted to library work, the rule of accuracy should include the rule that researchers must always determine the authoritativeness of their sources. Amateurs should also understand the limitations of using census data and nonscientific documents such as life records and newspaper accounts.

This is not an exhaustive list of such rules, of course. My point is that, except where the amateur is employed by a professional, we should attempt to train an exploratory observer—a part-time ethnographer who collects descriptive data—not a survey researcher, interviewer, or experimenter. This way, there is no need to be concerned with problems of entry, confidentiality or subject handling.⁷ If an amateur develops these latter skills, that is fine and a benefit to the field. The former, however, should be our goal.

This means there is no need whatsoever to immerse the amateur in statistics, methodology, theory, and the like—subjects that if pushed much are sure to keep membership in the local society at the lowest point possible. My research indicates that avocational scientists are sensitive about their educational gaps. Highly technical talks that they do not understand at monthly meetings, for instance, bore them and make them feel inferior.

Training depends to some extent on the background of the trainee. Given what passes for sociology in some off-campus bookstores (e.g., airports) and in popular culture (Mackie, 1975), I am convinced that screening procedures ought to include an interview with prospective members of the local society. Here their conceptions of the field could be determined and their reasons for joining the group established. Prospective members should be required to attend a minimum number of meetings

⁶ A number of amateur archaeologists mentioned with disgust the failure of certain professionals even to footnote the fact that the amateurs had gathered the data that the professionals found interesting enough to analyze and write up.

⁷ We might learn something of value from the amateur local historians in England who gather their data, among other ways, by interviewing the older inhabitants of the community for their reminiscences (Finberg and Skipp, 1967:44).

as a prerequisite for joining, to help ensure that they understand the group's aims.

It is hardly critical that amateur sociologists develop anything more than a general conception of their discipline. To view sociology as a profession does would require an ability at abstraction that most amateurs lack and have little interest in cultivating. Again, my association with other types of avocational scientists suggests that they operate quite effectively at their own level with only a sketchy understanding of what their science really is.

Since sociology, like other sciences, will probably attract more pure amateurs than any other kind, formal educational requirements should never be a membership criterion. Some of the most prolific amateur contributors to astronomy and archaeology, I discovered, have no university education in any field. Perhaps prospective members could read a particular paperback introduction to sociology or subscribe to *New Society* or *Society Magazine*. This could instill a certain level of understanding of the discipline. It could also promote a degree of solidarity, if all members—amateur and professional alike—shared this modest intellectual background. A book, whatever else it does, should provide a short and readable summary of the history of sociology and its main concepts.

Some consideration should be given to holding regular technical meetings along the lines of the "practical workshops" I observed in astronomy. Particular problems would be selected by the instructor for presentation to a small group of apprentices—problems such as how to take field notes, how to use a particular checklist or rating scale, or the limitations of autobiographical accounts. These workshops might include actual abbreviated field experiences during a particular evening meeting (as they sometimes do in astronomy). With technical matters more or less confined to these separate gatherings especially designed for apprentices, the main monthly meetings could be reserved for presentations of original data by masters and professionals and for talks of general interest to the entire membership.

It is unnecessary to go into the details of

routine organization of these societies, except to note that they would be run primarily by the amateurs themselves as is the practice in the other sciences.

Dissemination of Data

Presenting a talk at a meeting of the society or at a state, provincial, or regional conference is one way master amateurs in sociology could disseminate their observations. But since they would likely reach only a handful of professionals by this means, they should be encouraged to go beyond this and to put their data on paper. Some of the practical workshops could cover the tricks of the trade of sociological writing.

Written reports could be sent to the certain data clearing houses that now exist, providing the data fell within their frame of reference.⁸ More likely, however, reports would appear in the local society's bulletin, which could double as a newsletter (these are issued monthly, quarterly, or simply irregularly in other science societies). A university sociology department might publish some of the amateurs' observations as occasional papers.

These reports would summarize the results of an observational or library research project. They would contain what the observer recorded and little else. Unlike our journal articles, there would be no literature review, no theoretical organization of the data, no conclusion. There might be some discussion of observing techniques and problems, but only enough to acquaint the reader with the factors that influenced the observing process. The idea would be to encourage the dissemination of data, which would be facilitated by removing the writing impediments that frustrate so many professional sociologists.

⁸ There are at least two clearinghouses for social scientific data that appear likely to receive amateur reports. The National Clearinghouse for Mental Health Information in the United States receives reports and other documents on a wide range of topics related to mental health. The Data Clearinghouse for the Social Sciences, located in Ottawa, indexes quantitative data available in machine-readable form from Canadian social science research bodies. A case would have to be made for the acceptance by these clearinghouses of reports produced by amateurs; that case would need the support of professional sociologists.

An amateur might join with one or more professionals to publish a journal article based on the amateur's research. In other sciences outstanding amateurs occasionally manage to place their own articles in professional journals; this should also be encouraged in sociology.

WOULD SOCIOLOGY BENEFIT?

Yes, sociology would benefit. Promoting community interest in the discipline could lead to a more accurate popular conception of its nature. It could also help extinguish the tendency to confuse sociology with social welfare and psychology. In addition, amateur sociology could provide an arena where our undergraduate majors, who normally lose contact with the discipline once they graduate, could keep their interest alive. Many of them would undoubtedly become leaders in their local societies. It is possible, too, that the student careers of many undergraduates would acquire new meaning if they knew they could continue their major interest in an avocational vein beyond the university. And a continued avocational interest in sociology might enable them to develop and maintain an intellectual link between it and their occupations, which are typically nonsociological.

All sciences that have them recognize the recruitment potential of their avocational wings. Amateur sociology would be no exception. In time it would likely attract a number of high school students, as do the other sciences, some proportion of whom would choose to pursue a university education in the field. On a more crassly utilitarian level, amateurs add to the membership rolls of regional and national scientific associations.

A local society could also use the force of its numbers to promote sociology. For example, it might argue for the acquisition of certain books and periodicals at the public library, or raise and maintain the discipline's community visibility with repeated media coverage of its activities. It might even support a campaign by the national association for particular government policy changes, as long as a substantial majority of the members agreed with the association's position.

Public views about sociology would

have a better chance of reaching cloistered professional ears since they would be picked up by the amateurs as they go about their daily affairs in the community. These same amateurs could also speak for the discipline by correcting the misconceptions they encounter and by spreading the professional outlook on various issues.

Last, but hardly least, is the way an amateur sociology could benefit sociological theory and the identification of socially relevant research problems:

Certainly, a social science militia of this type would go far to complement, and perhaps to check, the social science army of professionals envisaged by the logistics discussed earlier. We would be overwhelmed with data: our poor schemes would have to be sturdy indeed to stand up to it—perhaps, afraid of drowning in data, we would become fonder of theory! We would also have to focus on questions that interested our militia—and work on the problem of easy communication with them. . . (Riesman, 1954:482)

CONCLUSIONS

A critical question remains to be answered: is there sufficient interest in any community to justify this proposal? Would there be members for an amateur sociology society should someone care to organize one? Let me start to answer this question by describing how the two science societies I studied got off the ground. The archaeological group began with approximately a half-dozen people who met sporadically in one another's living rooms for roughly ten years before incorporating in 1940 as the Dallas Archaeological Society (DAS). Today the society has 100 or so members. By contrast, the Royal Astronomical Society of Canada, Calgary Centre started up a decade ago at a meeting of nearly 200 people who assembled in response to a newspaper notice summoning those who were interested in amateur astronomy. The membership of this organization eventually stabilized at around 100, though it has since declined to between 80 and 90. Both societies were initiated by amateurs. Today, member dues are still the only source of support for their activities.

A local sociological society might begin

in either of these ways. But since there is no established amateur wing as in archaeology and astronomy, the impetus would have to come from interested professionals. From discussing this paper with students and nonuniversity friends and acquaintances and from presenting it at the University of Calgary and York University in Toronto, I have already identified enough enthusiasts to more than fill the standard living room. Like DAS, such a group would probably expand informally, by word of mouth and through the universities where its associated professionals are employed. At first the professionals would have to introduce the amateurs to appropriate research topics and data collection methods. Later the more sophisticated amateurs could pass on this knowledge themselves, thereby achieving a degree of independence from the professionals.

Why anyone would want to be an avocational scientist (including a sociologist) was addressed earlier when the rewards of amateurism were briefly listed. They are considered in detail in a separate paper (Stebbins, 1979).

Studying insects as a pastime revolts some people, while it clearly fascinates others. Undoubtedly, the same can be said for sociology. But whatever the field, avocational science is serious leisure, which offers benefits unavailable in the casual popular leisure of today. There are now hundreds of people in every North American city with bachelors degrees in sociology, and many more without them, all of whom have a gnawing curiosity about the human behavior they encounter as they go about their daily affairs. Some of these individuals find serious leisure to their liking, and there is no reason why sociology could not be one specific expression of this general interest, as history, entomology, mineralogy, and the rest have been.

Assuming that there are enough potential members in the typical urban community to comprise an amateur sociological society, what is the likely professional reaction to its presence? As a student of avocational science, it is my impression that a degree of hostility toward amateurs in our field will develop and persist as appears to have happened in every other

with an amateur component. Nonetheless, the literature bearing on amateurism in the six sciences (cited in the introduction), which is written largely if not wholly by professionals, is overwhelmingly in favor of such work.⁹

One of the common challenges to this proposal is that sociology is somehow different from those sciences. But have we not argued all along that conventional scientific method is the one we should adopt for sociology? That social science is essentially like physical science? According to that method, science as a process begins with careful exploratory observation of the phenomenon under study. From these data hypotheses are inductively formed and deductively tested through further controlled observation and experimentation. The amateur scientist is largely confined to making observations that the professional will use to generate and test hypotheses. If the observations are inaccurate, then the hypotheses will be falsified, providing the study is competently designed. This is one check on amateur work. Another is to have several amateurs collect data on the same phenomenon to determine the consistency of their observations.

Perhaps in the case of sociology this predicted hostility can be traced to the lack of an identifiable tradition of parallel amateur involvement. The idea simply sounds so foreign. Yet we sociologists have never had much love for tradition. In the seventies, with the discipline riding off in all directions, this proposal for an amateur sociology might just catch on. Tradition, if we ever had it, is at a low ebb; while amateurism as a complement to our professionalism is, I believe, an idea whose time has come.

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⁹ By the time this paper is published, I hope to have interviewed a sample of professional astronomers about the amateurs in their discipline.

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COMMENTS

In response to Stebbins' proposal I will pursue answers (initially given in parentheses) to these questions: (1) has an adequate analysis been offered here of amateur organizations in natural and social science fields? (no); (2) does the analysis contained in this proposal support the long-term viability of amateur sociology groups? (not by implication, though further analysis may); (3) could useful projects be conducted by expert amateurs who collect and report data for the use of professionals? (very definitely).

These answers and questions are obviously not mutually independent. How can I answer the last question affirmatively if, as Stebbins seems to assume, such projects are the foremost activity of amateur organizations? But then just how crucial are professional-amateur projects to the ongoing health of amateur groups? Suppose we consider the case of amateur astronomy.

Such projects have greatly stimulated the expression and further development of the technical knowledge of the most able amateur astronomers. This is consistent with Stebbins' emphasis on the science-doing activities of "master" amateurs. "Armchair" participants

of such groups are irrelevant to actual data-gathering projects. Recent conversations I have had with veteran members of the Indiana Astronomical Society and my own recollections of three amateur clubs some 15-20 years ago also support this view. The high points of my brief career as an amateur astronomer were the times I helped collect data I knew professionals would use. Now, as a sociologist interested in labor markets, I could make good use of information on various job markets in diverse occupations and organizations, which competent amateur observers might provide. Stebbins' idea of having observers comment on the fit of observations to theory also appeals to me.

However, amateur organizations seem comprised less by these master amateurs than by those armchair members who prefer to be vicariously associated with professional scientific activity. In all likelihood these members make up the bulk of the amateur readership of *Sky and Telescope* and the majority attending monthly meetings. Their primary interest is access to recent and "inside" information on current developments or discoveries. I do not see active research as the dominant orientation

of astronomy groups, though their leaders are usually capable of making observations for professionals' use.

My conversations and recollections suggest that too little collaboration between professionals and amateurs typically exists to sustain amateur organizations. Projects are intermittent at best, and less frequent than serious amateurs would like. The one possible exception to this is variable star observing, a continuous project for a very small number of amateurs which has virtually no impact on amateur groups. The Moonwatch program during the early years of satellites, grazing occultation observations, and the meteor observation program of the International Geophysical Year of the late 1950s illustrate the brevity of most projects. Thus collaborations with professionals cannot adequately account for the longevity of amateur astronomy groups.

If amateur organizations do not depend for their survival on such research projects, to what extent do the projects depend on organizations composed of expert amateurs and armchair participants? Professionals no doubt have recruited observers through amateur groups, and observers recruit other society members for projects; but observers can also be recruited independently, through printed media (e.g., *Sky and Telescope*) read by most serious amateurs. In short, there is a limited interdependency of professional-amateur projects and amateur societies per se. It will be interesting to learn whether Stebbins' conversations with professional astronomers confirm my observations.

At best, his proposal provides material useful for a needed analysis of the conditions under which amateur groups can generally sustain themselves. Two final comments on his proposal should suffice to indicate the inadequate analysis of these conditions it contains.

Stebbins does suggest "four conditions for the emergence and persistence of an avocational interest" (p. 240). They are not especially compelling or useful. In what fields can one not find beliefs in the existence of "undiscovered phenomena of scientific significance" (p. 240) or in the need for further systematic study of already known phenomena? Where is there not more work to do than professionals to do it? Condition three has possibilities: "phenomena, characteristics and behaviors . . . observable in their natural environment with the naked eye or with relatively inexpensive specialized equipment" (p. 240). But surely there are fields that satisfy these conditions without amateur organizations.

What about the possible effects of the content of different scientific fields on the sustenance of amateur groups? Rather than consider this issue, Stebbins chose to hoist sociology on

the petard of its often argued commonality with physical science. Suppose content affects the nature of knowledge and expertise which amateur experts attempt to gain. Suppose that this expertise has a unique technical and applied (rather than "pure science") content, such as, in amateur astronomy, mastering the equipment and procedures used for observation. I have looked for but cannot see a sociological counterpart to the amateur telescope maker or the expert par excellence of amateur astronomy. I do not see a sociological magazine for both amateurs and professionals (like *Sky and Telescope*) capable of helping galvanize the formation of local sociological societies.

However, a more thorough analysis, particularly of social science amateur groups (if there are any), may suggest a more sanguine prognosis for the latter-day successor to Mass Observation.

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Amateur sociology was the parent; professional, the child. Park was a professional writer, an amateur sociologist. He did join the sociological fraternity; to that extent he became professional. Sumner wrote a book; it got adopted by sociologists. Sumner accepted the adoption very unwillingly, and made no change in his writing or thinking. Small considered himself one of the creators of professional sociology.

At present sociology has a professional structure, and can control what is published as sociology, but thank God we cannot control what is written on sociological matters or in the sociological mood. Unless we get much more bureaucratized, it will always be possible for people to be *writers* of sociology. Of course, getting something published requires approval of someone with access to printing and circulation. The people who control popular publication have doctrine, canons of style; they will probably eventually be more stereotyped than the professionals. And they can't be voted out.

There's no virtue in being amateur. Researchers, professional or not, should try to publish their reports in the regular journals. In this way new lines of thought and research will reach the widest audience of interested readers.

In my career, I have seen ideas and lines of research opened up by people working in the margins of sociology; some of those ideas have

caught on and have enlarged and enriched our field.

I hope people will always work on the edge or clear outside the fences of sociology, not just for the hell of it, but out of curiosity and interest. Write in the sociological mode, and publish where you can. But let's not create professional amateurs.

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Stebbins' account of professional and amateur sociology unearths a set of boxes nesting in one another that seems to me to contain issues going to the foundations of current social science, conceptually and institutionally. I am interested in his proposal itself and its publication as data needing to be understood in their own right.

The most encompassing issue is embedded in Stebbins' assertion throughout that the discipline and profession of sociology exists, has definable and agreed aims, and is thereby a special stream in science as well as within social science. He presumes that there is some "accurate" version of sociology into which laypeople can and should be indoctrinated. Many institutional structures today support this version of the division of scientific labor—a version based more on professionalism and careerism than on ideas about the unity of science. Skepticism, so basic a tool in scientific observation and interpretation, is missing from this account of sociology. It is a skepticism our field should also be turning on itself. Should sociology be "promoted" as it is? Should we not examine those "misconceptions" of it as themselves sociological data?

The proposal mostly takes up issues of craft, unrelated however to the research questions for which the particular tactic of "volunteer" sociologists would be more useful than another. Stebbins provides examples from an "endless list" of observations needing to be made; endless is a far cry from significant. There is nothing new in temporarily expanding a workforce of census enumerators and interviewers for survey research—paid, of course. Respondents are often asked to keep detailed logs of their observations and activities. If there is some good reason for people to perform research tasks without pay Stebbins does not provide it—except to assert that people are enthusiastic about sociology.

My experience is different: I find people, untrained and trained, enthusiastically curious about the whys and wherefores of social life

and organization, but disdainful of the ways professional social scientists satisfy that curiosity. This is another major issue, the gap between social experience and sociological explanations. Riesman seems to me to be addressing some aspects of it: social science should be getting its definitions of research issues from laypeople as well; laypeople should be included in discourse with scientists for the natural knowledge their observations provide; there is so much more requiring explanation than shows in our maps of problematic social terrain.

Yet Riesman had professional preoccupations: his metaphor of "our militia" implies that we are the generals, they the troops. Why not they the generals, we the troops? Do astronomers fix the heavens and put the stars in place? To be controlled by what we could explain is simply a fundament of scientific inquiry—control of questions and methods. In social science, that principle becomes less simple because we have to be concerned as well with being comprehended by one another: the subjective meanings of social behaviors and events have to come coupled with all the other things we are able to explain them by. It is this complexity of which Stebbins is unaware in the trite parallels he draws with natural science. Hypothesis-testing is but one stage in scientific inquiry, not the whole of it. Why the social sciences continue to allow themselves to be tortured on this particular rack is another important question.

I was asked to comment on this proposal partly because of another proposal I have developed for a new institution—a Center for American Field Studies. This center would specialize in ethnographic reports of ongoing social situations, e.g. the structure and operations of organizations and markets, occupations and households, courts and schools. These reports would simultaneously be sources of data about American culture, my particular interest: the tacit assumptions, shared understandings, categories, conventions, definitions, and rules Americans use in a wide range of settings. The reports would be made at the request of other scientists whose own interests and skills do not include ethnography but whose research standards and plans require such grounding. My proposal is based on the specific strengths of the anthropological perspective (available to all scientists) in making visible subjective understandings, implicit premises, and underlying dynamics (systems of invisible incentives and sanctions and their consequences, behavioral adaptations, understood hierarchies of prestige and rank, and the systematic description of "anomalous" behavior, for example). Journalists do that kind of reporting extremely well,

when they do it; theirs is by definition an enterprise of turning up leads, not following them up, analytically or pragmatically.

The need in social science for a better balance with questions of incidence and distribution partly prompted my proposal for a naturalistic basis for the study of complex, industrial society. I am proposing as well to provide an alternative to research perspectives uninterested in what people already know and use as the basis of their behaviors as members of groups, that is, the observations and interpretations of all of us as "amateur sociologists." Our observations and chains of reasoning are the natural data controlling the investigation of culture as a scientific enterprise. Unlike Stebbins, I am interested in people's "conceptions of the field," not for screening them in or out of a local society but as a source of data important to the further development of interpretive social science. Not "toward amateur sociology"—rather, thank goodness, there is no getting away from it.

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This is an excellent paper as it stands. However, readers might want to consider the following comments:

The British organization, Mass Observation, existed in a relatively nonintrusive society, with (as compared to the United States) greater concern for one's own and other people's privacy. (There is a great mania about one's own privacy in the United States, but no comparable zeal to inhibit invasions by seemingly legitimate others or to control voyeurism.) Canada and the United Kingdom may in this respect have more built-in defenses against the risks of some kinds of amateur sociology than the United States would have.

However, Stebbins already anticipates this objection by requiring amateurs—used in the old-fashioned sense of that term—to be supervised and to start with a certain amount of instruction and proper cautionary comments. By engaging primarily in the use of unobtrusive measures and observations, the amateur will avoid the risks of harm to which Stebbins is already sensitive. One of the public areas in which I have encouraged such unobtrusive observation is in churches, where the observer in outward appearance is similar to the congregation and hence not seen as an intruder.

I have had a fair amount of experience with undergraduate amateurs of the sort described.

At least in a research-oriented university, such amateurs and potential apprentices may need a good deal of encouragement to believe that they can contribute anything to what appears to them as a towering and self-confident structure of established knowledge. Here Stebbins is quite right that publication is essential; I had to publish three volumes of undergraduate papers before being able to persuade students, especially non-majors or lower-division students, that they could make a contribution.

While many students are shy about what they can contribute, I have also known of those who might exploit "passing" as a sociologist to try to get the goods on the "bad guys," or engage in crusades, thus making entry in the future more difficult if not impossible. Stebbins is well aware of this problem. The vast number of survey organizations now in existence, and the problem survey researchers have of being mistaken for salesmen (because salesmen may pose as survey researchers) means that work of this sort must be supervised. Thus, to join an amateur society the screening interview seems essential, as well as other testimonies concerning scientific seriousness, such as attendance at a certain number of meetings.

Stebbins' analogies with archaeology and astronomy are excellent; I am marginally familiar with the role amateurs have played and play in both fields, the hardships endured, the avocational rewards. These are also present, as is mentioned in passing, in the field of oral history or contemporary folklore where, as Stebbins knows, reportage should not be confused with validated judgments.

In my experience, a few undergraduates with experience in sociology have become sufficiently interested in the topic they have investigated to come into the field as professionals and in a number of cases, to make the topic the subject of their dissertations and indeed of later work. The individual student or non-student adult is well served by the self-confidence that comes from making this kind of contribution. The hardships are different from those suffered by archaeologists or astronomers, but they do involve nerving oneself to become an observer, the arduousness of keeping careful field notes (I have sometimes encouraged the keeping of personal diaries as well) and the meticulous description of events.

Orally, if not in writing, Everett Hughes has talked about "fire house research," by which he means going down the greased pole in the fire station and out to an ongoing, unanticipated event. Disaster researchers usually come in after the event; a network of amateur sociologists could be mobilized at the time of a black-out, assuming that telephones still worked, to observe the way various groups and individuals conducted themselves.

Among amateur musicians, as among amateur squash and tennis players, but in a still more formalized way, there is a handbook which lists and rates, for example, cello or bassoon players by a rough ABC-type grading. This means that in a mobile society, musicians arriving in a new community can find their peers and level of performance. To my knowledge, nothing of this sort exists in Europe or elsewhere. One can conceive of similar gradations, partly self-descriptive and perhaps in some fashion reviewed, for a network of amateur sociologists in a local area.

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There are many attractive features to what Robert Stebbins proposes. After all, who would not like to have a constituency out there among the general public who would provide admiration and possible political support? Wouldn't we all like to have our data collection expanded by a dedicated force that could go beyond what we can wrest from unwilling students and could be afforded by our increasingly stingy granting bodies? Besides, there are some places and topics that our data collection techniques do not seem to reach well, and possibly amateurs might do better.

But can amateurism be developed as an auxiliary force for sociology? What would motivate someone to become an amateur sociologist? Are there observations amateurs could make that would be of intrinsic interest to them? And how would such observations differ from the stuff of everyday experiences and rubberneck tourism? Are there topics and issues to which we could guide amateurs and for which we could provide easy-to-use methods that would go beyond what they already experience? Are there discoveries out there? Are there analogues to collecting pre-Columbian Indian artifacts or filling in star maps?

I doubt whether we have any tasks to offer that would be interesting to amateurs. After all, the sociologist's advantage is the ability to transcend everyday experiences, to show the distributional aspects of human behavior and to provide an explanation of process. We have little to discover, but much to organize into coherent descriptive and explanatory schemes.

The Mass Observation movement is an interesting case in point: the main objective of MO was to support the British war effort in World War II by providing the Home Office with information about the wartime concerns

and worries of the British population. To participate as an observer in MO, listening to conversations in bars, in workplaces, on public transport, and so on, was to participate in the war effort. (And how much different is that from providing information on statements of popular disaffection to the authorities in police states?)

And then there is the anecdote problem: what is to prevent amateur sociologists from being collectors of odd facts, interesting and startling experiences, miscellaneous overheard conversations and the like? All the anecdotal evidence that plagues sociologists at cocktail parties may be gathered and displayed by the amateurs at their meetings. Stebbins recognizes this problem and would establish the principle that anecdotal evidence would not be allowed in amateur sociological societies. But, then what would amateurs observe and collect? Would he have them conduct surveys and/or depth interviews? Count interactions? Measure worker productivity in the amateurs' workplaces? Collect job histories and occupational genealogies? Would interview schedules and interaction protocols become the analogues of the telescope and tweezers?

The issue boils down to this: there are some sciences, e.g., astronomy, zoology, archaeology and entomology, to which amateurs can and have contributed on the level of considerably extending the scope of basic data through observation. There are other sciences, e.g., physics, chemistry and biology, where amateurs may have once played important roles but no longer do so. Is sociology more like the former disciplines which have large descriptive tasks as part of their scientific missions; or is sociology more like the latter, which are concerned more with models of processes?

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REJOINDER

Though a certain number of critical comments have been made about this proposal, I am prepared to rest my case; to let it stand on its own merits without hashing what I meant to say and what particular reviewers thought they read. Rather, I wish only to offer a single clarification of a poorly communicated passage that caught Althausen's attention. It concerns the four conditions that encourage the emergence and persistence of an avocational interest in a science.

When I wrote this passage I had Kuhn's concept of "normal science" in mind. Amateurism takes root in those sciences or areas of sciences that lack a dominant paradigm as the exclusive guide to scholarly research. These fields are in a preparadigm stage of development in which several schools of thought compete. Here there is an awareness among professionals of conditions (1) and (2): that there are significant undiscovered phenomena and that many important characteristics and behaviors of known phenomena have yet to be systematically studied. In other words, the need as the professionals see it is for more descriptive data, a requirement they find they are unable to fill without help. If their field also satisfies condition (3), then the stage is set for the development of an avocational wing. It should be clear that I see sociology as such a preparadigm science in which these conditions are met.

Why then, is there no amateur sociology? Among the answers to this question are that we lack a tradition of amateurism, a vision that amateurs could help us with some of our research interests, and a willingness to organize nonprofessional assistance on any basis other than temporary. The same conclusions could likely be reached for other sciences; even though they meet the four conditions, they still have no amateur practitioners.

The present state of knowledge about the avocational sciences is so sketchy that even their extent is unknown. Since there is typically scant awareness of these pastimes in the larger community (ham radio and astronomy being partial exceptions), it is difficult to learn how widespread they actually are. I am surprised at how my own list continues to grow. For example, the July/August, 1978, issue of *Loisir Plus* (p. 15) lists the sciences with local and regional societies organized under the Fédération Québécoise du Loisir Scientifique (Quebec, Canada). These include, in addition to the sciences mentioned in preceding proposal, botany, geography, horticulture, mathematics, meteorology, mycology, and zoology.

In short, the issue, raised by Althaus, of how well my proposal explains the way avocational science groups emerge and persist will be settled when we can identify the fields with amateur developments and compare them with fields that lack such developments. This issue, however, is of interest chiefly to sociologists of science. It in no way vitiates the present call for an amateur sociology, even though detailed knowledge about the emergence and persistence of avocational science groups would certainly make it easier to heed that call.

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ENVIRONMENTAL SOCIOLOGY: A NEW PARADIGM?*

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William R. Catton, Jr., and Riley E. Dunlap (1978) have recently advanced a "New Environmental Paradigm" (NEP) as the cutting edge of environmental sociology and perhaps the discipline as a whole. In so doing they argue that "ostensibly diverse and competing perspectives in sociology are alike in their shared anthropocentrism" (1978:41), i.e., the similarities of these previous competing theories overshadow their apparent dissimilarities. Catton and Dunlap term the

old paradigm, which cuts across the many groupings of competing theories, the "Human Exceptionalism Paradigm" (HEP). To them, the HEP is not only anthropocentric, but also unrealistic and inappropriate for understanding the fundamental ecological substratum of human societies—the survival limits of human populations in the biosphere. My argument is that neither the NEP nor the HEP embodies a coherent set of domain assumptions, and as a result, the assumptions of each are neither internally consistent nor mutually exclusive. I do not seek to deny the relevance of the distinction that Catton and Dunlap draw; I suggest rather that the HEP/NEP cleavage is part

* I wish to thank Allan Schnaiberg and Charles Geisler for their useful comments on a previous draft of this paper. [Address all communications to: Frederick H. Buttel, Dept. of Rural Sociology, Cornell University, Ithaca, NY 14850.]

of an ongoing, lively debate *within* major extant sociological paradigms, and that the resolution of this debate depends upon the paradigm in question.

The HEP/NEP Distinction

Catton and Dunlap argue that practitioners of various theoretical postures in contemporary "mainstream" sociology share four HEP assumptions. The authors refer to the HEP molded by these assumptions as a basically "optimistic worldview." But it is unclear how this worldview adds up to a paradigm that has fostered or will foster a coherent, cumulative (although possibly flawed) line of research and theoretical elaboration. The optimistic worldview cum HEP does indeed embody a *general*—usually implicit—assumption that societal adaptation to the confines of the biosphere is not particularly problematic. However, the HEP "paradigm" as presented by Catton and Dunlap does not specify—nor do I feel that the range of theoretical thought encapsulated within the HEP will allow them to specify—the general or specific modes that this presumed adaptation is likely to take. Implicit within the HEP model of adaptation of course is the notion that material progress is possible and that the barriers to this progress are primarily social—not biogeochemical. Yet I would argue that the roots (i.e., "world hypotheses" or "domain assumptions"; Gouldner, 1970:30–35) of this optimistic image of social and material progress are so diverse that it is illusory to emphasize only their similarities and minimize their differences. There are many distinct sociological viewpoints that Catton and Dunlap group under the HEP rubric; there is, for example, the enduring philosophical/epistemological debate about whether this material progress can realize its full expression within capitalism¹ (cf. Aronowitz, 1974 and Bell,

1973). Further, Aronowitz's work clearly casts doubt on the validity of the authors' assertion that "persistent adherents of the HEP [are] accustomed to relying on endless and generally benign technological and organizational breakthroughs" (1978:45). Aronowitz's analysis of the technological and organizational parameters of capitalist society is hardly a benign one. In sum, the HEP does not offer a coherent set of domain assumptions that can specify the fundamental dynamics or "laws of motion" of societies; clearly, HEP sociologists' differences in this regard can best be depicted in terms of the traditional theoretical groupings—functionalism, positivism, Marxism, and so forth—that Catton and Dunlap find of little use.

A final observation on the HEP is that Catton and Dunlap might well be ascribing more consistency to the four HEP assumptions than actually is the case. In particular, it is not clear that assuming the unique cultural character of human societies and the diversity of cultural expressions leads to any necessary assumption about the lack of limits of survival base or the perpetuity of present trajectories of technological and material "progress." The four HEP assumptions, then, are more loosely organized than Dunlap and Catton argue—further depreciating their presumed leading role in shaping sociological inquiry.

The assumptions underlying the NEP reflect a more "ecologically realistic" worldview than those of the HEP. Catton and Dunlap are correct that most environmental sociologists readily accept the premises of limits and ecological constraint, and I agree with the importance of incorporating these assumptions into the mainstream of sociological analysis. However, the critical question in my view is whether environmental sociologists—defined as those who agree on all or most NEP assumptions—represent the fundamental theoretical consonance that is ascribed to them.

To repeat some of the illustrative citations made by the two authors, I would

¹ Thus the HEP (as well as the NEP) abstracts beyond and provides no insight into longstanding domain assumptions concerning issues such as the extent to which "society is precariously or fundamentally stable; that social problems will correct themselves without planned intervention . . . [or] that man's true humanity resides in his feelings and sentiments" (Gouldner, 1970:31). Also unclear is the

relation of the HEP and NEP to the issue of the autonomy of social structure (Gouldner, 1970:51–54).

argue that the perspectives of Charles Anderson (1976) and Allan Schnaiberg (1975, 1977), on one hand, and Samuel Klausner (1971) and William Burch (1971), on the other, diverge fundamentally, although all four environmental sociologists are solidly within the "NEP" tradition. I am inclined to characterize the major contours of these divergences as being between "critical-Marxist" and "functionalist-organicist" NEP practitioners (see Buttel, 1976; Buttel and Flinn, 1977, for related schemas). Nevertheless, my contention here is that sharing the three NEP assumptions does not presuppose either theoretical consonance or cumulative inquiry on the part of environmental sociologists. The working domain assumptions of the NEP and environmental sociology are still matters of controversy.

In this regard it is worth special mention that Catton and Dunlap tend to group academic Marxists as being supporters of unbridled economic expansion and critics of environmentalism—in other words, non- or anti-environmental sociologists. Again, I feel that there is ample evidence that a substantial minority of Marxists have quite readily adopted their overall theoretical posture to a meaningful (NEP-based) environmental sociology (see, for example, Anderson, 1976; Ezenberger, 1974; Molotch, 1976; England and Bluestone, 1973; Deutsch, 1976; Applebaum et al., 1976).

Environmental Sociology and Social Stratification

Catton and Dunlap's application of the NEP to the analysis of social stratification, class structure and the sociopolitical morphology of conflict illustrates the limits of assigning paradigmatic or pre-paradigmatic status to environmental sociology. The authors' primary point is that existing U.S. economic institutions are fundamentally implicated in an expanding, nonredistributive treadmill. Why this is the case is unclear from the three NEP assumptions set forth by Catton and Dunlap. Again, this ambiguity seems to arise because the NEP assumptions have little to say about the dynamics and laws of motion underlying socioeconomic transformation.

Both the dominant class and the working class tend to find their interests best served by economic expansion (or the promise of future expansion), even though the working class absorbs the bulk of the "costs" (especially pollution and the destruction of urban residential environments) of such expansion (Burch, 1976; Schnaiberg, 1975; England and Bluestone, 1973; Molotch, 1976; Morrison, 1976). Dominant class support for economic expansion and general opposition to recognizing the limits of the biosphere are quite understandable, because corporate profits (and the salaries of high-level managers) are directly or indirectly based on growth and environmental destruction (Hardesty et al., 1971). But since the impacts of environmental reform ("planned scarcity" in Schnaiberg's [1975] terms) tend to be substantially regressive (see also Sills, 1975), the subordinate classes tend to join their more privileged counterparts in pressing for further economic growth (Schnaiberg's [1975] "economic synthesis" of the societal-environmental dialectic). However, following NEP logic, the economic synthesis is fleeting and temporary because it implies exponential economic and materials growth in a finite planet. Many environmental sociologists are now coming to realize that a "sustainable society" (Stivers, 1976; Pirages, 1977; i.e., Schnaiberg's "ecological synthesis") requires a far more egalitarian social structure than presently exists in the U.S. In other words, *redistribution* in some fashion seems to be a prerequisite for breaking the chains of repeated return to the ecologically disastrous economic synthesis (see also Stretton, 1976).

I tend to agree with the basic outlines of this analysis and Morrison's (1976) earlier presentations of its major elements. However, I feel that Catton and Dunlap's analysis of stratification and class structure remains incomplete in one important respect. Given that movement toward the ecological synthesis is necessary to achieve a sustainable society, and that redistribution is in turn necessary to achieve the ecological synthesis, the question becomes how can we best conceptualize and help bring about these social changes. Catton and Dunlap argue that the American Left (trade unions, spokespersons for

the poor, and Marxists) has abandoned hopes for "real" redistribution in favor of settling for a "fair share of a growing pie." My concern is that Catton, Dunlap, and others have abandoned the Left as a progressive force for achieving the ecological synthesis. I would argue that the Left is the only group (or congeries of groups) that can unite around "real" redistribution; the dominant class (including environmentalists at the fringes of that class) can hardly be expected to support such a program of redistribution and environmental control. To count out the Left is to ignore dominant class interests in the hegemony of growth (Molotch, 1976) and the possibility that Marxian-type conflicts may be necessary to bring about the transition to the sustainable society that environmental sociologists ostensibly advocate. This difference between Catton and Dunlap, and me, on the possible role of the Left, is the basis of my argument that all possible NEP-informed analyses of social stratification and class structure are not directly deducible from NEP assumptions.

In this regard it is interesting to note that Catton and Dunlap agree with Schnaiberg (1975:9-10) that "the synthesis adopted will be influenced by the basic economic structure of a society, with 'regressive' (inequality-magnifying) societies most likely to maintain the 'economic' synthesis and 'progressive' (equality-fostering) societies the least resistive to the 'ecological' synthesis." Sweden, a society with a long history of class conflicts that can be legitimately regarded as proto-typical Marxian-type conflicts, and with lower levels of inequality as a result of these more heightened class antagonisms (Anderson, 1976:246-251), is suggested by the authors to be farthest along the road toward the ecological synthesis. This circumstance should suggest some reconsideration of the present and, especially, the future role of the working class and the American Left in securing ecologically beneficial changes (see Deutsch, 1976). This is not to say that all manifest or latent ecologically beneficial initiatives must necessarily come from the Left or that the Left must inherently be a demonstrable pro-environmental force. Rather, I wish to suggest the importance

of realizing that since the subordinate class derives the smallest benefits and highest costs from the economic synthesis, the Left is logically the nascent force for redistribution and ecological protection—the ecological synthesis.

Conclusion

Catton and Dunlap's paper deserves great attention because it does alert us to some key concerns of environmental sociologists and major issues that face both society and sociology. However, I believe that environmental sociology is not and can never be a paradigm in the sense of producing a cumulative, coherent literature. More specifically, the sets of assumptions associated with the HEP and NEP "paradigms" have virtually nothing to say about the key social forces leading to change and transformation. The HEP/NEP cleavage, to be sure, *is* real, but *within existing paradigms* (Friedrichs, 1972). Thus I do not wish to deny the validity of the HEP/NEP distinction, but rather to indicate that the intellectual "action" is equally lively within, for example, Marxist and functionalist circles, as well as between NEP sociologists and their HEP opponents generally. I do believe that environmental sociology is more than just another sociological sub-area such as political sociology or the sociology of religion, but this does not in itself confer paradigmatic importance upon environmental sociology. The emergence, then, of sociological inquiry in the NEP tradition need not lead to a reduction of the influence of existing sociological paradigms; in fact, existing work in environmental sociology leads me to believe that fundamental differences may well be reinforced. In sum, environmental sociology in my view will continue to be pervaded by theoretical—that is, paradigmatic—diversity.

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PARADIGMS, THEORIES, AND THE PRIMACY OF THE HEP-NEP DISTINCTION

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A paradigm, as we have used the term, is an image shared by members of a scientific community telling them the nature of their science's subject-matter. Theories are not paradigms, nor does each paradigm generate one and only one theory. Yet Buttel mistakenly equates

"theoretical diversity" with "paradigmatic diversity."¹

¹ This mistake can be attributed to the varied usage of the word "paradigm" in sociological and other literature. For examples of diverse meanings quite unrelated to Kuhn's (1962), see Gross

Given a particular paradigm, certain kinds of questions are askable and certain kinds of hypotheses are conceivable. But a paradigm (Gestalt, Weltanschauung) is not so specific that a coherent and detailed theory must follow logically and uniquely from it (Ritzer, 1975:4-7).² Thus Buttel's comments about coherence and specificity do not show, as he supposes, that differences between critical-Marxism and functionalist-organicism are more paradigmatic than differences between the New Environmental Paradigm and the Human Exceptionalism Paradigm.

In our previous paper (Catton and Dunlap, 1978:42-43) we suggested that sociologists of various theoretical persuasions had heretofore accepted at least implicitly four assumptions we called the HEP.³ These four assumptions obviously do not constitute a theory. We called them a paradigm because they represent a *world-view* that has been so common among sociologists as to make it "difficult to recognize the reality and full significance of the environmental problems and constraints we now confront" (Catton and Dunlap, 1978:44).

Ironically, this difficulty is exemplified even in literature intended as an antidote to disciplinary stagnation, such as the volume of essays entitled *The Uses of Controversy in Sociology*. In their introduc-

tion, Coser and Larsen (1976:xv) begin by quoting Claude Bernard who once said, "It is what we think we know that prevents us from learning." By certain lacunae, however, the essay collection seems to illustrate this very point. Unless most sociologists have indeed taken for granted the ideas we call the HEP, how can one account for this volume's omission of any essays about environmental influences upon human society, about the effects of energy and technology upon social organization, about the relation between social phenomena and biological processes, or about the adequacy or inadequacy of natural resources to support burgeoning populations and resource-hungry technology? These topics arise from the ecosystem-dependence of human societal life (which HEP-based thought does not recognize). Controversies about them were already raging before 1976, but not yet among mainstream sociologists.

The nearest approximation to any encounter with such concerns in the *Uses of Controversy* volume appears in David Riesman's essay, "Some Questions about Discontinuities in American Society" (3-29), when he mentions (p. 26) Robert Heilbroner's *An Inquiry into the Human Prospect* (1974) while discussing the erosion of authority and the inability of industrial societies to control simultaneously inflation and unemployment. But Riesman conveys nothing of the gravity of Heilbroner's book and merely seems to exemplify what Bernard meant, for he only says that since Heilbroner "is a reflective political economist, I find his work more persuasive than that of ecologists and demographers engaged in extrapolating the Malthusian variables." He does not mention Heilbroner's (1974:132, 136) anguished expectation that a period of "convulsive change" has become inevitable, nor Heilbroner's (1974:142-143) dread that our resource-ravenous generation, imbued with the "Promethean spirit" may "curse . . . future generations whose claims to life can be honored only by sacrificing present enjoyments" and may "condemn them to nonexistence by choosing the present over the future." Instead, Riesman moves quickly on to comment on matters much less grave than such a human prospect.

(1967:viii, 83, 230). For discussion of other misleading usages by sociologists, see Ritzer (1975:19-24). Kuhn himself, despite ambiguous usage of "paradigm," never equated it with "a theory" (see Masterman, 1970:61-67).

² For example, as Ritzer (1975:25-26) has shown, both "structural-functionalism" and "conflict" theory stem from the same "social facts paradigm." Similarly, others have argued that these two competing theoretical perspectives share a paradigmatic conception of societies as "holistic systems." Without citing Kuhn, whose influence had not yet begun to permeate sociology, van den Berghe (1963) described "similarities in outlook" that transcended "differences in emphasis" between functionalism and Hegelian-Marxian dialectic.

³ Since writing the earlier paper we have become aware of a criminologist's perceptive articulation of five paradigmatic assumptions implicit in sociology which cause sociological criminologists to neglect influences of the physical environment. Their similarity to the HEP is clear (see Jeffery, 1976:152). Likewise, standard assumptions in economics and political science are being challenged by ecologically informed scholars (see, e.g., Daly, 1977; Ophuls, 1977).

Scarcely any ecologists (sociological or biological) are cited in the volume, and the name with the greatest number of index entries is Karl Marx. Though Otis Dudley Duncan has several listings, they are for his work with Blau on occupational structure rather than for his efforts to show sociologists the relevance of the ecosystem concept (e.g., Duncan, 1964). The name index has room for a number of nonsociologists, including political figures like John Foster Dulles, Mahatma Gandhi, Ralph Nader, and George Wallace. But, as if no controversies pertinent to sociology had arisen from issues they expounded, there is no mention of Rachel Carson, Barry Commoner, Paul Ehrlich, or Garrett Hardin. In the subject index, none of the following terms appears: ecology, energy, environment, impact assessment, limits to growth, pollution, population pressure, resources, scarcity. But then, neither are there references to development, poverty, or the Third World. To us, these conspicuous omissions seem symptomatic of an obsolete paradigm, blinding sociologists to matters of increasing sociological import.

Buttel's emancipation from that traditional worldview is substantial; he is one of the sociologists who do recognize environmental *problems* as real and fraught with social significance (see Buttel, 1976:307-308). But he has revealed an unfortunate ambivalence. His recognition of environmental *constraints* is clouded—apparently by his desire to believe in the adequacy and efficacy of redistributive solutions to the social aspects of environmental problems. Redistribution, he insists, is prerequisite to "breaking the chains of repeated return to the ecologically disastrous economic synthesis" (p. 268) of Schnaiberg's (1975) societal-environmental dialectic. We are not at all unsympathetic to redistribution, but we believe it has generally connoted (to advocates adhering to the HEP) a process of "leveling up." On the other hand, the NEP calls attention to circumstances in which redistribution may have to mean "leveling down," due to ineluctable resource limits, etc. (see, for example, Ophuls, 1977:20-139). Buttel stresses subordinate classes' resistance to "managed scarcity" owing to the tendency of

managed scarcity's burdens to be regressively distributed. The NEP, however, should enable sociologists to ask whether the feasibility of redistribution may be jeopardized by the resistance *it* will incur (from all classes but the very lowest) if ecological limits compel it to become a leveling down operation.

Further, the NEP reveals a new dimension of "redistribution"—the competition Heilbroner discerned between people now living versus posterity. This diachronic competition besets all industrial societies, committed as they are to massive dependency upon exhaustible resources. It may be even harder to resolve than problems of equity between social classes (synchronic competition). Buttel has not taken this longitudinal dimension into account in assessing what kinds of conflicts may be required to attain a "sustainable society." What he terms "Marxian-type" conflicts may not be, after all, the most serious ones we face.

Cleavage between NEP-adherents and HEP-adherents is more fundamental, we have said, than cleavages that differentiate various HEP-based theories. Theoretical groupings such as functionalism, exchange, Marxism, etc. within contemporary sociology do differ, and for some purposes the differences clearly matter. However, the developing NEP-HEP cleavage cuts across these theoretical perspectives. For example, both Aronowitz (1974) and Anderson (1976) are "critical-Marxist" theorists who are aware that our society is undergoing tremendous change, yet Anderson's NEP-oriented analysis of the changing conditions differs markedly from the implicitly HEP-oriented analysis of Aronowitz. The contribution of ecological limits to our current problems, and the Promethean curse upon posterity (diachronic competition), are visible to Anderson but not to Aronowitz. We see a similar NEP-HEP cleavage developing within Marxist economics. For example, Enzensberger (1974) acknowledges and attempts to come to grips with ecological limits, an idea foreign to traditional Marxist thought (see Ophuls, 1977:8, 116, 145, 204).

Reasserting one's preference between left and right ought not to require one to underestimate the enormity of the

paradigm shift represented by emergence of the NEP.⁴ Adherents of NEP will continue to differ among themselves when it comes to policy proposals for coping with ecosystem-based social problems (compare, e.g., Anderson, 1976 and Ophuls, 1977), but we are convinced the NEP is increasingly necessary for accurate perception of the ecological constraints and environmental factors influencing human societies.

⁴ Recognizing the diversity among environmental sociologists, we probably should have qualified our reference to the "New Environmental Paradigm" . . . implicit in environmental sociology" (Catton and Dunlap, 1978:42). All environmental sociologists share an interest in interactions between environment and society, but we should have stipulated that not all have either consciously or implicitly adopted the NEP. For elaboration of the types of societal-environmental interactions studied by environmental sociologists, see Dunlap and Catton, 1978.

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